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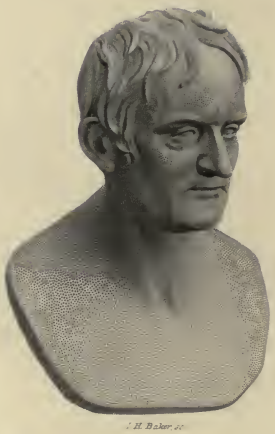

J. H. Baker sc.

JOHN DALTON

From the Statue by Chantrey

MEMOIRS

OF

THE LIFE AND SCIENTIFIC RESEARCHES

OF

JOHN DALTON,

HON. D.C.L. OXFORD; LL.D. EDINBURGH; F.R.S.;
PRESIDENT OF THE LITERARY AND PHILOSOPHICAL SOCIETY, MANCHESTER;
FOREIGN ASSOCIATE OF THE ROYAL ACADEMY OF SCIENCES, PARIS;
MEMBER OF THE ROYAL ACADEMIES OF SCIENCE OF BERLIN AND OF MUNICH,
AND OF THE NATURAL HISTORY SOCIETY OF MOSCOW,
&c. &c. &c.

By WILLIAM CHARLES HENRY, M.D., F.R.S.,
FELLOW OF THE CHEMICAL AND GEOLOGICAL SOCIETIES; AND CORRESPONDING MEMBER OF THE ROYAL ACADEMY OF SCIENCES, TURIN.

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TO

PROFESSOR HENRY ROSE,

OF THE UNIVERSITY OF BERLIN,

IN TESTIMONY OF PROFOUND RESPECT FOR HIS PRE-EMINENT SERVICES IN THE ADVANCEMENT OF ANALYTICAL CHEMISTRY;

AND OF GRATEFUL REMEMBRANCE OF HIS INVALUABLE INSTRUCTIONS, AND OF NUMBERLESS OFFICES OF KINDNESS,

THESE PAGES ARE AFFECTIONATELY INSCRIBED

BY HIS ATTACHED FRIEND AND FORMER PUPIL,

THE AUTHOR.

PREFACE.

It seems necessary to explain in few words the circumstances which induced me to undertake the difficult task of becoming Dalton's biographer, and which have so long delayed its accomplishment.

Dr. Dalton, in the first clause of his will, bequeathed to me and to his three executors "all his philosophical, scientific, and literary manuscripts and correspondence, to be disposed of as they may judge most fit;" and in a codicil to the same, he bequeathed to me all his chemical and other philosophical instruments and apparatus. I received intelligence of these bequests in August 1844, at Paris, on my way to Italy, where I remained till the following autumn. Regarding them as significant of my venerable friend's intention that I should act as his literary executor, and should write some account of his life and discoveries, I commenced shortly after my return to prepare for the task, by the careful re-perusal and analysis of all his published works. I also applied to Mr. Peter Clare, the acting executor, into whose hands the manuscript remains of Dalton had fallen, for those

documents. Mr. Clare, who had for many years stood in most intimate relations with Dr. Dalton, and placed the highest value on the papers in his possession, manifested on the numerous occasions when I applied for them, personally and by letter, great reluctance to resign them to me. On one occasion he proposed a joint authorship of Dalton's life, which I declined. He finally promised to bring the documents to me in the country, on the ground that they needed his oral comments; a promise he was never able to fulfil. After his decease they were forwarded to me by the surviving executor, Mr. Neild, to whom I am also indebted for promptly and courteously imparting to me all further information to which he had access. Early last spring, shortly after I had received the manuscripts, and indeed before I had been able to inspect them, Mr. Graham did me the honour to invite me, on the part of the Cavendish Society, to write a Life of Dalton for their volumes. This flattering invitation determined me at once to undertake the task.

The MSS. remains of Dalton consist of his early scientific journals, chiefly devoted to meteorology; of note-books, containing records of short excursions in England and Wales; of a correspondence which he had maintained with his brother, Jonathan Dalton, a schoolmaster at Kendal, between the years 1793—1824; of a few letters from men eminent in science;

and of a letter-book, into which he had copied all important letters written by him in the years 1836—42. Of these documents the letters addressed to his brother throw most light on his scientific researches and habits of life. I have applied to all scientific persons, likely to be in correspondence with Dalton, for his letters, but with only limited success. Mr. Jonathan Otley, of Keswick, a few months Dalton's senior, and still surviving him, has contributed several letters and an interesting narrative of their joint mountain excursions in the Lake District. I have been favoured by Mr. I. F. Crosthwaite with a valuable series of letters, addressed to his grandfather, Mr. Peter Crosthwaite of Keswick, relating chiefly to meteorological observations. I am also indebted to Mr. Giles, Mr. Woolley, and Mr. Wilkinson, for permission to peruse and extract from biographical notices of Dalton, read by them before the Literary and Philosophical Society of Manchester, and still unpublished. Mr. Woolley has also most obligingly placed at my disposal the sketch of Dalton's domestic habits by the late Miss Johns, in whose father's house Dalton was an inmate during a considerable part of his life. Dr. Davy, besides permitting me to publish inedited notices of Dalton by his brother, Sir Humphry Davy, and by himself, has with the greatest kindness procured for me

many interesting particulars respecting Dalton's early life, as a schoolmaster, in Kendal. Among my father's papers also I discovered many valuable documents; as notes of conversations with Dalton, and several letters from Mr. G. W. Wood, Mr. Babbage, and other persons respecting Dalton's pension. These materials had been collected and preserved by my father with a view of their being made available for a future biography of Dalton, a work which he was desirous that I should undertake. My friend Mr. Joseph A. Ransome has favoured me with a narrative of Dalton's illness and death, and with an interesting letter, detailing his own reminiscences of Dalton.

The only published notices of Dalton which I have consulted are those by Dr. Thomson, in his *History of Chemistry*, and in the *Proceedings of the Glasgow Philosophical Society;* that by Dr. Hermann Kopp;* a short account of his life, said to have been corrected by himself, which appeared in Wheeler's *History of Manchester;* and especially an elaborate and well-conceived article in the *British Quarterly Review*, vol. i., by Dr. G. Wilson, the accomplished biographer of Cavendish,—beyond comparison the ablest and justest appreciation that has yet appeared of Dalton's philosophical character and discoveries. To Dr. G. Wilson I am also indebted for

* *Geschichte der Chemie*, theil i. p. 362.

the frank and liberal communication of his views respecting colour-blindness, and of all sources of information known to him respecting Dalton.

Dr. Dalton's life was marked by few external events. Its main incidents were mental efforts, resulting in signal discoveries. To accomplish the clear enunciation of these, which constitute the staple material of his biography, I have been compelled to depart from a strictly chronological order. For Dalton, it will be seen, grappled in early years with several fundamental problems in the philosophy of heat and meteorology, and carried about with him these favourite speculations, bound up in his inmost nature, during a long life; attracted to them again and again, either by the workings of his own thoughts or by the critical notice of his contemporaries. Though averse to change, he often materially modified his original conceptions. In order, therefore, to avoid the confusion of constant recurrence to the same questions, I have deemed it expedient on the first appearance of each of his master ideas, excepting the Atomic Theory, to trace its subsequent development and to exhaust the subject before breaking fresh ground. I have also endeavoured, as far as consisted with my own limited knowledge, to supply the corrections or confirmations, which the ideas and experimental results of Dalton have subsequently received, and to define

their present scientific appreciation. I have striven to estimate his services to science, calmly and impartially: but I cannot affect to be insensible to the memories of the past—of his almost life-long friendship with my father, never shadowed by even a passing cloud; or of his early favourable notice of and unceasing benevolent regard towards myself, thoughtfully manifested in his last bequest to me of what he had most prized in life.

HAFFIELD,
March 3, 1854.

CONTENTS.

CHAPTER I.

PAGE

Birth and early education—Keeps a School at Kendal with his Brother, 1785-1793 — Contributions to the Diaries — Intercourse with Mr. Gough—Meteorological Journal—Correspondence with Mr. Crosthwaite —Contemplates studying Medicine—Lectures at Kendal—Settles in Manchester, 1793 1

CHAPTER II.

Publishes his Meteorological Essays — Observations on the Aurora Borealis—Subsequent Memoir on same subject—Colonel Sabine's Letter—Joins the Literary and Philosophical Society of Manchester—Essay on his peculiarity of Vision of Colours—Letter from Sir John Herschel—Essays on quantity of Rain and Dew—On the power of Fluids to conduct Heat—Maximum Density of Water—Heat and Cold produced by Condensation and Rarefaction of Air—Constitution of Mixed Gases—Force of Steam—Evaporation—Expansion of Gases by Heat 17

CHAPTER III.

Uneventful Life—Extracts from Letters to his Brother—Elements of English Grammar—Visits to Lancaster and the Lakes—Lectures at the Royal Institution, 1804—Experiments on Atmospheric Air—Law of Multiple Proportions—Subsequent Memoirs on the Atmosphere, 1826 and 1837—Diffusion of Gases—Absorption of Gases by Water—First Table of Atomic Weights, 1803—General Survey of the First Decennial Period of his Life in Manchester—First Part of New System of Chemical Philosophy Published, 1808 — Laws of Temperature, refuted by Petit and Dulong, and finally abandoned by Dalton—Specific Heats of Atoms—Foreshadowings of the Atomic Theory . . . 40

CHAPTER IV.

PAGE

Atomic Theory—Views of Wenzel, Richter, Proust, and Berthollet—Anticipations of Dr. Bryan Higgins and Mr. Wm. Higgins—Their Works not known to Dalton—Law of Multiple Proportions Discovered by Dalton—Steps leading to the Atomic Theory and Determination of Atomic Weights—Compound Proportions—Chapter on Synthesis—Number of Atoms in Compounds—Dr. Thomson and Dr. Wollaston earliest Converts—Sir H. Davy's Objections—Gay-Lussac's Law of Volumes—Dr. Kopp's Explanation of Dalton's Rejection of this Law—Researches of Berzelius—Letter from Berzelius to Dalton . . 71

CHAPTER V.

Atomic Theory, continued—Confirmation by subsequent Discoveries—Isomorphism, Electrolysis, Specific Heat—Equality of Atoms in Gases—Isomerism—Dr. Wollaston's Argument from the Finite Extent of the Atmosphere—Objections considered—Table of Atomic Weights—Relation to that of Hydrogen—Other Numerical Relations—Atomic Diameters calculated by Dalton—Atomic Volumes—Kopp and Schröder—Dalton's Symbols—Organic Chemistry—Rational Formulæ—Law of Substitution—Chemical Types—Law of Homologues—Atomic Spirit of Dr. Hofmann's Lectures—Opinions of Berzelius, Mitscherlich, Graham, Gmelin, and Herschel—Letters from Faraday and Liebig . 102

CHAPTER VI.

Publication of Part II. of Volume I. "New System"—Potassium and Sodium and Chlorine Controversies—Letter from Sir H. Davy—Law of Volumes—Charcoal—Essays on the Phosphates, Salts of Ammonia, Sulphuric Ether, Oil Gas, and Meteorology—Mr. Otley's Account of their joint Mountain Excursions in the Lake District . . 135

CHAPTER VII.

Offer of an Appointment in the Polar Expedition—Elected Corresponding Member of the French Academy of Sciences—Fellow of the Royal Society—Visits Paris, 1822—Extracts from his Journal—Mr. Dockray's Letter and Account of a Dinner at Laplace's—Royal Medal—Davy's Anniversary Discourse—Publishes 1827 Volume II. of "New System"

PAGE

Appendix most valuable——Foreign Associate of the French Academy, 1830—Attends the Meetings of the British Association at York and Oxford—Receives the Honorary Degree of D.C.L. at Oxford, 1832—Pension, 1833—Doubled, 1836—Professor Sedgwick's Speech at Cambridge—Statue by Chantrey—Degree of L.L.D., Edinburgh—Presentation at Court—Mr. Babbage's Letter 161

CHAPTER VIII.

His Brother's Death—Dublin and Bristol Meetings—First Attack of Paralysis, 1837—Lord Burlington's Address to the Liverpool Meeting—Papers on the Phosphates and Arseniates, on Microscomic Salt, Mixture of Sulphate of Magnesia and Biphosphate of Soda, Water of Crystallization and Analysis of Sugar—Important Discovery confirmed by MM. Playfair and Joule—Dr. Playfair's Summary of their Main Results—Meeting of British Association in Manchester, 1842—Lord Francis Egerton's Notice of Dalton—His Death, July 27, 1844—Mr. Ransome's Account of his Illness—Public Funeral—Will—Foundation of Scholarships and Bronze Statue 190

CHAPTER IX.

Personal Appearance—Portraits and Statue—Moral Excellences—Anecdote from Dr. Playfair—Miss Johns's Notice of his Domestic Habits and Character—Extracts from Mr. Giles's Sketch of his Life—Notices by Sir H. Davy and Dr. Davy—Letter from Mr. Ransome—General Survey of his Main Discoveries, and of his Characteristic Mental Endowments 205

Appendix. On Colour-Blindness, by Dr. George Wilson of Edinburgh . 239

LIFE OF DALTON.

CHAPTER I.

BIRTH AND EARLY EDUCATION—KEEPS A SCHOOL AT KENDAL WITH HIS BROTHER, 1785–93—CONTRIBUTIONS TO THE DIARIES—INTERCOURSE WITH MR. GOUGH—METEOROLOGICAL JOURNAL—CORRESPONDENCE WITH MR. CROSTHWAITE—CONTEMPLATES STUDYING MEDICINE—LECTURES AT KENDAL—SETTLES IN MANCHESTER, 1793.

John Dalton was born on the 5th of September, 1766, at the village of Eaglesfield, near Cockermouth, Cumberland. "This village is said to contain the first meeting-house established by the Society of Friends in England. It is now used as a school-house, and its ancient burial ground is overgrown with long grass." His family, for three previous generations, are known to have resided here, and to have possessed a small copyhold estate, which became the property of the philosopher on the death of his elder brother, Jonathan, in 1834. The house is described by Miss Johns, who visited it with Dr. Dalton in 1834, as "one of the better class of farm-houses of that district; it had undergone considerable alterations, though its identity was by no means effaced." Towards the close of his life, when scientific honours and a national reward had been bestowed upon him, he appears to have traced with interest the lowlier fortunes of his ancestors, and to have carefully drawn out and preserved, on a parchment pedigree, all that could be ascertained respecting them. From this document, it seems that Jonathan Dalton, his grandfather, was the first of the race who

joined the Society of Friends, a religious body to which his descendants zealously adhered. The pedigree is surmounted with the armorial bearings of the family of Dalton, emblazoned in their proper colours; and records their alliances with the Greenups and the Fearons, which latter family purchased a messuage and tenement at Eaglesfield in the reign of Elizabeth. Both the Daltons and Fearons belonged to that class of respectable yeomen, or small landed proprietors, called in the Lake district "statesmen," which is now almost extinct.

Nearly all that is known respecting his early training and occupations, is embodied in a short notice by an intimate friend, which was submitted for correction or confirmation to Dalton himself, and was published in Wheeler's *History of Manchester*. To this sketch I am indebted for many valuable facts. In a contribution to Mr. Roberts's *Book of Autographs*, Dalton has thus himself briefly recorded his early history:—

"The writer of this was born at the village of Eaglesfield, about two miles west of Cockermouth, Cumberland. Attended the village schools, there and in the neighbourhood, till eleven years of age, at which period he had gone through a course of mensuration, surveying, navigation, &c.; began about twelve to teach the village school, and continued it two years; afterwards was occasionally employed in husbandry for a year or more; removed to Kendal at fifteen years of age as assistant in a boarding-school; remained in that capacity for three or four years, then undertook the same school as a principal, and continued it for eight years; whilst at Kendal, employed his leisure in studying Latin, Greek, French, and the mathematics, with natural philosophy; removed thence to Manchester in 1793 as tutor in mathematics and natural philosophy in the New College; was six years in that engagement, and after was employed as private and public teacher of mathematics and chemistry in Manchester, but occasionally by invitation in London, Edinburgh, Glasgow, Birmingham, and Leeds.

"*Oct.* 22, 1832. "John Dalton."

It is manifest, that for his mental culture, Dalton was mainly beholden to his own efforts. He was emphatically self-taught; and from his very boyhood, were implanted those habits of stern self-reliance, of indomitable perseverance, and of severe, patient concentration of thought, by which, in after-life, he was often heard to affirm, rather than by the suggestive inspirations of genius, he had slowly wrought out what he fitly termed "A NEW system of Chemical Philosophy." His earliest tastes impelled him to study the relations of space and number. "It is related, that when about ten years old, his curiosity was excited by a dispute among some mowers, as to whether sixty square yards and sixty yards square were identical; at first he concluded that they were, but after-reflection showed him that they were not. About this period he was taken notice of by a distant relative, Mr. Elihu Robinson, a gentleman of some property, in whose service was a young man named Alderson, of double Dalton's age. Mr. Robinson, himself a gentleman of liberal education, had an accomplished wife, and under their joint direction, Alderson and Dalton pursued their studies together. Mr. Robinson used to relate, that when any difficult question in mathematics was proposed, Dalton, with the resolute perseverance which has so strongly marked his character, far from being daunted, used to encourage his companion, by remarking, in the dialect of his country, "Yan might do it." On one of these occasions, Alderson proposed to settle a dispute with Dalton by a wager of sixpence; but Mr. Robinson put his veto upon the proposition, and suggested, that instead of risking money, the losing party should undertake to supply his companion with candles for the nights' studies during the winter. The suggestion was adopted, and Dalton won the wager."* Alderson was still living at the advanced age of ninety, and was visited by Miss Johns and Dr. Dalton in 1834. "He apologized much for his not having the power to entertain us. He was exceedingly proud and pleased that the Doctor had not forgotten his old friend. 'Ah, you see, ladies,' he tremblingly

* Wheeler's *History of Manchester*, p. 500.

exclaimed, 'he is not only a great but a good man, to think of such a humble person as I—he who has just been introduced to the King.'"

At the age of twelve, as recorded above, he actually commenced a school in his native village; and persevered in teaching during two winters.

John Robinson, a very aged man, born, bred, and still living at Eaglesfield, remembers Joseph Dalton, the father, and Deborah, his wife, who were married 1755, and had three children, Jonathan, John, and Mary. "The father," he says, "was a weaver by trade, and still rather poor." John Robinson was a pupil at Eaglesfield school, and tells of the little authority Dalton had over his scholars; how they challenged him out into the graveyard to fight, many of them being the age of himself; and how they broke the school windows after he had locked them in to get off their tasks at dinner-time. "When I was a boy I saw John Dalton at my cousin William Alderson's house in Eaglesfield; they talked of days when they were lads together, sitting over the fire till midnight, boring at the *Ladies' Diary;* John never giving sleep to his eyelids until he had found out the riddle of some prize enigma, or some mathematical question."—*Letter from Mr. James Dickinson to Dr. R. A. Smith, Manchester.*

At the same time, his bodily frame was invigorated by sharing in the labours of his father's farm; and in this simple life, all his inborn faculties, physical and mental, unfolded themselves healthfully and energetically. In the year 1781, being then fifteen years old, he left Eaglesfield for Kendal, to join his cousin George Bewley, who, with Jonathan Dalton as an assistant, conducted a school for members of the Society of Friends. I have before me a printed notice, that this school would be re-opened on the 28th of March, 1785, by Jonathan and John Dalton; "where youth will be carefully instructed in English, Latin, Greek, and French; also writing, arithmetic, merchant's accounts, and the mathematics." The terms did not exceed the sum of ten shillings and sixpence per quarter; and were afterwards, in 1811, raised by Jonathan Dalton to fifteen shillings, not without an apologetic

hope, that a small advance (the increased price of the necessaries of life considered) would not be thought unreasonable.

Mrs. Cookson, who became a pupil at the Kendal school in the summer of 1785, has kindly communicated her reminiscences of her former masters to me through the medium of Dr. Davy. "The school was not generally popular, owing to the uncouth manners of the young masters, who did not seem to have had much intercourse with society; but John's natural disposition being gentler, he was more passable. I believe the last time of my going to Mr. Dalton was about the year 1789. He was then become rather more communicative in his manner, but still a man of very few words." The number of scholars might have been about sixty boys and girls. Jonathan, the elder brother, was considered the principal. He was stern and severe in manner; John was milder. Great regularity was observed, any improprieties were noted down; a mark on paper was put against the name of the culprit; and the remark in a solemn manner was often made, by one or other of the masters, that 'the day of reckoning would surely come.' Only on one occasion was corporal punishment inflicted. This was on three boys who disregarded admonition and warning. The punishment was a most severe whipping on the naked skin, inflicted by Jonathan with the aid of his brother, who held the boys. The blood was made to flow, and in brief, the aid of a surgeon was afterwards required. The severity of the measure gave rise to much discussion; and the boys punished would have been taken from the school, had not a strong interest been used in support of the masters. The teaching of the brothers is described as strictly elementary, and quite in the ordinary way. Never in the reading lessons any remarks or comment. John Dalton was considered very studious. He often made calculations on fragments of paper. The girls found such torn and thrown-by near his desk. The brothers led a very secluded life. Their manners must have been more than usually hard and ungainly."—*Letter from Dr. Davy.*

Mr. Isaac Braithwaite, who was a pupil at the Kendal school at the same period, thus confirms Mrs. Cookson's

impression. "He recollects the boys being all glad to be taught by John Dalton, because he had a gentler disposition; and besides his mind was so occupied with mathematics, that their faults escaped his notice. His brother, who divided with him the management of the school, was more vigilant."

In Kendal he found somewhat larger means of self-improvement, and a rather wider field for pursuing his early chosen and lasting career of teacher. Here he resided, conducting the school, in association with his elder brother, for a period of twelve years. During this period he contributed frequently to two periodical works then in considerable repute, the *Gentleman's and Ladies' Diary*. The volumes from 1784 to 1794 contain many solutions of questions in mathematics or general philosophy, to which his name is attached. He obtained two of the prizes awarded by the editors.

An essay of great value and interest, entitled "*An Account of the Early Mathematical and Philosophical Writings of the late Dr. Dalton,*" has been recently read to the Manchester Society by Mr. T. T. Wilkinson, F.R.A.S., of Burnley, and is destined for publication in their volumes. Mr. Wilkinson has kindly permitted me to consult and extract from it. The selection of questions for the year 1787, embraced nearly all the branches of mathematics then cultivated by English geometers; and yet he correctly solved thirteen out of the list of fifteen, the prize question included. His solution of question 850 is inserted at length in the Diary, and is probably the earliest printed specimen of his mathematical writings. He was equally successful in the following year, 1788; and from his replies to questions in general philosophy, appears to have already bestowed some attention on chemistry, and to be conversant with some French writers on that science. Mechanics and fluxions had also engaged his attention. On the appearance of the *Ladies' Diary* for 1789, Mr. Dalton must have felt himself amply rewarded for all his previous disappointments; for besides obtaining insertion of his answers to *all* the philosophical queries, and to *three* out of *eleven* solutions, sent to the questions in the mathematical department, he was awarded

the "prize of six diaries." In the *Gentleman's Diary* for the same year his name is announced as having furnished correct solutions to seven of the mathematical questions, of which that to question 591 relating to a case of hydrostatical equilibrium, is inserted at length, and gained him his first position amongst the correspondents to that noted and difficult serial. "The *Ladies' Diary* and supplement for 1790 conveyed the gratifying intelligence that he had been awarded the highest prize of ten diaries for his masterly solution of the prize question." Dr. G. Wilson of Edinburgh supplied the following extracts from the *Ladies' Diary*, containing some amusing queries and solutions by Dalton on questions not connected with mathematics.

"I. *Query by Mr. William Gradidge, of Canterbury.*

"Whether, to a generous mind, is the conferring or receiving an obligation, the greater pleasure?"—*Diary* for 1791, page 32.

"*Answered by Mr. John Dalton, of Kendal.*

"The pleasure arising from conferring an obligation, especially if it be effected without much inconvenience, is pure, and must be a grateful sensation to a generous mind; but that arising from receiving an obligation is often mixed with the unpleasing reflection of inability to remunerate the benefactor. It is pretty clear, therefore, that the pleasure of conferring an obligation must exceed that of receiving one."—*Diary* for 1792, page 24.

"II. *Query by Mira.*

"Is it possible for a person of sensibility and virtue, who has once felt the passion of love in the fullest extent that the human heart is capable of receiving it, (being by death, or some other circumstance, for ever deprived of the object of its wishes) ever to feel an equal passion for any other object?"—*Diary* for 1791, page 32.

"*Answered by Mr. John Dalton, of Kendal.*

It will be generally allowed that in sustaining the disappointments incident to life, true fortitude would guard us

from the extremes of insuperable melancholy and stoic insensibility, both being incompatible with your own happiness and the good of mankind. If, therefore, the passion of love have not acquired too great an ascendency over the reason, we may, I think, conclude that true magnanimity may support the shock without eventually feeling the mental powers and affections enervated and destroyed by it, and consequently that the query may be answered in the affirmative. However, if this passion be too strong, when compared with the other faculties of the mind, it may be feared that the shock will enfeeble it so as to render the exercise of its functions in future much more limited than before."—*Diary* for 1792, page 24.

"III. *Query by Miss Nancy Linson, of York.*

"Why does sugar dissolve sooner in cold water than in spirits?"—*Diary* for 1791, page 32.

"*Answered by Mr. Dalton.*

"Probably spirits dissolve sugar solely by reason of the water they contain, and this being only a part of their composition, renders the solution more slow than when the whole menstruum is pure water."—*Diary* for 1792, page 25.

"IV. *Query by Mr John Dalton, of Kendal.*

"What is the cause of the mist which is sometimes observable in a calm evening, especially in summer, to hover over rivers, meadows, &c.?"—*Diary* for 1793, page 32.

"*Answered by the Proposer, Mr. John Dalton, of Kendal.*

"This phenomenon is only observed when the air has suffered a sudden change of temperature from heat to cold. It is found (from experience) that warm air will hold more water in solution with it than cold air; therefore where the air is suddenly cooled, which sometimes happens in an evening, the water being then much warmer than the air, it evaporates pretty copiously at the surface, but is no sooner carried up a little into the cold air, than it is precipitated again in the form of a mist, and occasions the phenomenon."—*Diary* for 1794, page 24.

But this epoch of his life was rendered especially instructive and memorable by his close friendship with Mr. John Gough, towards whom, in the Preface to the first edition of his *Meteorological Observations and Essays,* 1793, he has thus warmly expressed his sense of obligation :—"To one person, more particularly, I am peculiarly indebted, not only in this respect, but in many others; indeed, if there be anything new and of importance to science contained in this work, it is owing, in great part, to my having had the advantage of his instruction and example in philosophical investigation."

The following extract from a letter of Dalton's to Mr. Peter Crosthwaite, Keswick, conveys a more detailed notice of Mr. Gough :—

"Kendal, April 12, 1788.

"John Gough is the son of a wealthy tradesman in this town; unfortunately he lost his sight by the small-pox, when about two years old, since which he has been quite blind, and may now be about thirty : he is perhaps one of the most astonishing instances that ever appeared, of what genius united with perseverance and every other subsidiary aid, can accomplish, when deprived of what we usually reckon the most valuable sense. He is a perfect master of the Latin, Greek, and French tongues; the two former of which I knew nothing of six years ago, when I first came here from my native place near Cockermouth, but under his tuition have since acquired a good knowledge of them. He understands well all the different branches of mathematics, and it is wonderful what difficult and abstruse problems he will solve in his own head. There is no branch of natural philosophy but what he is well acquainted with; he knows by the touch, taste, and smell, almost every plant within twenty miles of this place; he can reason with astonishing perspicuity on the construction of the eye, the nature of light and colours, and of optic glasses; he is a good proficient in astronomy, chemistry, medicine, &c. &c. His father being very able, has furnished him with every necessary help, and would have sent him to the university, had he been inclined. He has

the advantage of all the books he has a mind for, which others read to him; he employs one of his brothers or sisters to write for him, or else myself, especially in his mathematical attempts; he has never studied the art of writing much, or I doubt not he would succeed. He and I have been for a long time very intimate; as our pursuits are common—viz. mathematical and philosophical—we find it very agreeable frequently to communicate our sentiments to each other, and to converse on those topics."

On publishing a second edition of this work in 1834, after the death of Mr. Gough, he was able and eager to praise, without reserve, the rare accomplishments of his early friend and instructor.

"At the conclusion of my former preface, I alluded to a person who had laid me under great obligations. That gentleman being now no more, I can speak of him without reserve. It was the late John Gough, Esq., of Kendal.

"Mr. Gough might justly be deemed a prodigy in scientific attainments, considering the disadvantages under which he laboured. Deprived of sight in infancy (one or two years old) by the small-pox, he was destined to live to an advanced age, under this, which is commonly reputed one of the greatest misfortunes that can fall to the lot of man. In his case, however, it may fairly be questioned whether he would have had more enjoyment in himself, and have been of more use to society in the capacity of a manufacturer, his probable destination, than in that which was allotted to him. By the liberality of his father, he had the benefit of a good classical and mathematical education; and naturally possessing great powers of mind, he bent them chiefly to the study of the physical and mechanical sciences. There are few branches of science in which he did not either excel, or of which he had not a competent knowledge; astronomy, optics, pneumatics, chemistry, natural history in general, and botany in particular, may be mentioned. For about eight years during my residence in Kendal, we were intimately acquainted. Mr. Gough was as much gratified with imparting his stores of science as I was in receiving them. My use to him was

chiefly in reading, writing, and making calculations and diagrams, and in participating with him in the pleasure resulting from successful investigations; but, as Mr. Gough was above receiving any pecuniary recompense, the balance of advantage was greatly in my favour; and I am glad of having this opportunity of acknowledging it. It was he who first set the example of keeping a meteorological journal at Kendal."

His subsequent relations to Mr. Gough furnish a pleasing manifestation of Dalton's right feeling and generous temper. For, many years after his leaving Kendal, when his essays on evaporation and the constitution of mixed gases had raised him to high rank among the cultivators of science, Mr. Gough had taken occasion to controvert the doctrines of his former pupil with some asperity of language. Dalton, however, not unmindful of former kindnesses, though evidently pained by the temper of Mr. Gough's criticism, did not permit himself to retaliate upon one whom he regarded as his benefactor. He contented himself with remarking that "in discussions relative to experimental philosophy, we expect facts opposed to facts and arguments to arguments; whereas, in the present instance, Mr. Gough has done little more than insinuate that my instruments are inaccurate and the results of my experiments unfaithfully represented, without in any one instance bringing either of these charges home to me." He replied with unruffled calmness to all that bore the character of argument, continued in friendly relations with Mr. Gough, and finally, after the death of that gentleman, thus placed on lasting record his grateful sense of past acts of friendship and his profound respect for his instructor's remarkable mental gifts.

In the last passage quoted, he attributes to Mr. Gough the having first set the example of keeping a meteorological journal at Kendal. Dalton himself commenced one, entitled *Observations upon the Weather*, &c. &c., March 24, 1787. I find the first entry for that day:—"In the evening, soon after sunset, there appeared a remarkable aurora borealis, the sky being generally clear, and the moon shining; it

spread over above one-half of the hemisphere, appeared very vivid, and had a quick vibratory motion; about eight, the heavens were overcast and the aurora almost disappeared. N.B. Three nights before, a similar aurora appeared, with rather a brisker wind, and the day following windy and stormy." It is not improbable that the appearance of this aurora impelled him to commence his journal; and it is not without interest, that his first meteorological observation should relate to a class of phenomena which occupied his attention much in after-years, and respecting which he has published novel and important matter. During nearly six months, his journal consists of general remarks on the state of the weather. He then begins to record, in a tabulated form, the indications of the thermometer, barometer, and a rude kind of hygroscope, and notes: "The preceding and following observations on the temperature of the weather, were made on instruments of my own construction. The barometer is graduated into sixteenths of an inch. The thermometer is mercurial, with Fahrenheit's scale, exposed to the open air, but free from the sun. The hygroscope is about six yards of whip-cord, suspended from a nail, with a small weight to stretch it; its scale, tenths of inches, beginning from no certain point,—the less the number, the shorter the string and the greater the moisture." I have in my possession two volumes of this journal, comprising the years 1787—93 in Kendal, and 1793—1803 in Manchester. It was continued with steady perseverance in unbroken sequence to the evening before his decease.

I am indebted to Mr. J. F. Crosthwaite, of Keswick, for permitting me to examine an interesting series of letters from Dalton to that gentleman's grandfather, the late Mr. Peter Crosthwaite, founder of the well-known museum of that town, in the years 1787—94. They relate almost entirely to meteorological observations, which were made simultaneously by the two friends at their respective stations, Kendal and Keswick, for the purpose of comparison. Dalton, it appears, supplied Mr. Crosthwaite with a barometer and thermometer of his own construction, for which he charged the modest

sums of 18*s.* and 5*s.* It is true that the barometer was not a very refined instrument, for, in a letter to Mr. Crosthwaite, May 24, 1788, he describes minutely the mode of its construction. It is obvious, that, as he omits to boil, or even heat the mercury after it is poured into the tube, both air and moisture must remain attached to the tube and mingled with the mercury. This imperfection he seems to have discovered, for he writes soon afterwards:—"I intend to renew mine as soon as convenient; if thou do the same be careful in undoing it, and attend to the cautions I give; be sure to rub the inside of the tube well with warm dry cotton or wool, and have the mercury when poured in at least milk-warm; for moisture is above all things else to be avoided, as it depresses the mercury far more than a particle of air does; mine is, as I have said, at least $\frac{1}{16}$th of an inch too low, and yet it is clear of air, and to all appearances dry; but I doubt not but attending to these precautions, *which I knew nothing of when it was filled,* will raise it up to its proper height." Again, in January, 1793, he observes:—"I consider both our barometers as inaccurate with respect to the distance of the *basins and scales;* but this is of little importance, provided they be true in other respects; this only serves to show the relative heights of the places to the sea, which we can come at better by other means."

Botany occasionally furnishes a topic in this correspondence. Dalton informs Mr. Crosthwaite that he had "dried and pressed a good many plants, and pasted them down to sheets of white paper, and found that they look very pretty, and attract the attention of all, both learned and unlearned; this has induced me to think, that a tolerable collection of them, treated in this manner, would be a very proper object in the museum. I cannot say what kind of a recompense would be equivalent to such a task, but think I could engage to fill a book of two quires for half a guinea." He afterwards writes, October 4, 1791:—"I have at length completed the book of plants, and made an index both to the Linnæan and English names. I am not so confident in my abilities, as to maintain that I have given no plant a wrong

name, but I believe the skilful botanist will find very few, if any, miscalled." Mr. Isaac Braithwaite remembers, that once when Dalton was botanizing with a companion, "they had a narrow escape from a bull that attacked them in a field; Dalton saved himself by climbing into a tree, or over the wall."

Among his papers of this early period is a small quarto volume, entitled, "Philosophical Memoirs, begun at Kendal, 1787, auctore Johanne Dalton." It is loosely attached to two similar books, which carry down the history of his inquiries to the close of 1801. This journal records little of interest, between June 21, 1787, and the end of that year, except the measurement of the heights of some hills near Kendal, by means of the barometer. The year 1788 commences with a "Memorandum of the going of two hygrometers, or pieces of whip-cord, each being 8 feet 5 inches long, stretched by equal weights, and similarly situated along an oaken post in the school, where was no fire." These experiments are followed by a table of times when the auroræ boreales have been seen, together with the moon's age at the several times. His attention was also occasionally occupied, in the years 1787-9, with observations on the changes of caterpillars, and on the power of a vacuum or immersion in water to destroy or suspend vitality in snails, mites, and maggots. In sending to his correspondent, Mr. Crosthwaite, specimens of butterflies and ichneumon flies, for the museum, he observed, "they may perhaps be deemed puerile, but nothing that enjoys animal life or that vegetates, is beneath the dignity of a naturalist to examine." Early in the year 1790 he performed an elaborate series of experiments on his own ingesta and egesta, in order to ascertain the weight lost by insensible perspiration: these he published, forty years afterwards, in the memoirs of the Manchester Society, vol. v., p. 303. He there states, "During my residence at Kendal, nearly forty years ago, I had at one time an inclination to the study of medicine, with a view to future practice in the medical profession. It was on this account chiefly, but partly from my own personal interest in

knowing the causes of disease and of health, that I was prompted to make such investigations into the animal economy, as my circumstances and situation at the time would allow." These observations were commenced, March 13, 1790, and after being continued for fourteen days, were resumed in June and September of the same year. It would appear, that at this period he had entertained a serious design of relinquishing the profession of a schoolmaster, and entering upon the study of law or medicine. In his papers are preserved replies from his worthy cousin, Mr. Elihu Robinson, of Eaglesfield, and his uncle, Thomas Greenup, residing in Devonshire Street, London, dated April 10th and 9th, 1790, to letters, in which he had consulted them on this subject. Mr. Greenup bluntly replies: "As to the two professions of law and physic, if thou wishest to be at the head of one of those professions: that is, to be at the bar or to be a physician; I think they are both totally out of the reach of a person in thy circumstances. . . . . If thou art tired of being a teacher, and wishest to change it for some more lucrative or agreeable employment, and couldst be content, instead of becoming a physician or barrister, to move in the humbler sphere of apothecary or attorney, thou mightest, perhaps, be able, with a little capital and great industry, to establish thyself in one of these."

Elihu Robinson, of a tenderer nature, and with a juster appreciation of his relative's endowments, replies: "As I have thought thy talents were well adapted to thy present profession, I cannot say thy proposal of changing it was very welcome to me: believing thou wouldst not only shine but be really useful in that noble labour of teaching youth." . . . . . He concludes: "Now, after using so much freedom, I may own, I doubt not but thy genius, unshaken perseverance, and steady application, may gain a competent knowledge in any profession, and I am far from thinking that of physic would be a misconstruction or misapplication of thy talents, parts, or genius. So I much desire thou mayst be guided *by best wisdom* in all thy pursuits." His determination to remain faithful to his earliest profession issued, doubtless, out of

that prudent caution, and wise, yet generous economy, which characterized him throughout life. The world has certainly no cause to regret his decision; for, in the words of Playfair, "a man of talents may follow any profession with advantage; a man of genius will hardly succeed but in that which nature has pointed out."

During the winter of 1787, he delivered, at Kendal, a course of twelve lectures on natural philosophy, comprising mechanics, optics, pneumatics, astronomy, and the use of the globes; a course which he repeated in 1791, with the addition of a lecture on fire. His terms of admittance to his second course were exceedingly moderate, being sixpence each lecture, or five shillings the whole,—half the sums announced for the earlier course. Possibly he may not have found in the Kendal of the last century, a sufficiently large audience at the higher fee. From this period it became a part of his regular occupations and an important source of his slender revenues, to deliver lectures in Manchester and elsewhere.

In the spring of 1793, he was invited, mainly in consequence of Mr. Gough's favourable recommendation, to join a college established in Manchester, in the year 1786, by a body of Protestant Dissenters, as tutor in the department of mathematics and natural philosophy. The terms proposed and accepted by Dalton were, that he should receive three guineas per session from each student attending his lectures, with the condition that the sum shall not fall below 80*l.* per session of ten months. Commons and rooms in the college were allotted him at 27*l.* 10*s.* per session. He resigned this appointment after a period of six years; but continued to reside in Manchester during the whole of his subsequent life.

CHAPTER II.

PUBLISHES HIS METEOROLOGICAL ESSAYS — OBSERVATIONS ON THE AURORA BOREALIS—SUBSEQUENT MEMOIR ON THE SAME SUBJECT—COL. SABINE'S LETTER—JOINS THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER — ESSAY ON HIS PECULIARITY OF VISION OF COLOURS — LETTER FROM SIR JOHN HERSCHEL—ESSAYS ON QUANTITY OF RAIN AND DEW—ON THE POWER OF FLUIDS TO CONDUCT HEAT—MAXIMUM DENSITY OF WATER—HEAT AND COLD PRODUCED BY CONDENSATION AND RAREFACTION OF AIR—CONSTITUTION OF MIXED GASES — FORCE OF STEAM — EVAPORATION — EXPANSION OF GASES BY HEAT.

It was shortly after Dalton's removal to Manchester that he published his first work, entitled "Meteorological Observations and Essays." The materials, however, had been collected, and the work entirely written and printed, at Kendal.* Indeed his residence, during the first twenty-six years of his life, among the lakes and mountains of Cumberland, and his early familiarity with the ever-varying conditions of the atmosphere, as respects heat and moisture; with the deposition of vapour on the colder summits in the form of cloud, and its speedy re-absorption and disappearance when drifted into the warmer valleys, doubtless impressed the first special direction on his genius; and thus, as will afterwards appear, determined his subsequent course of research and discovery. We learn from the preface to the first edition, September 21, 1793, that his "principal design was to explain the nature of the different instruments used in meteorology, particularly the barometer and thermometer. . . . . Soon after this, having discovered the relation of the Aurora Borealis to magnetism, I found that, in order to establish the discovery, a pretty large dissertation would

* See MS. Notes of Conversation with Mr. Dalton, 1830, February 13, by the late Dr. Henry (p. 62).

be required, which must of course be addressed more peculiarly to philosophers." He was induced to print a second edition of this work in 1834, verbatim from the first, with the addition of a few notes, entitled "Appendix to the Second Edition." "I have been the more anxious (he says) to preserve the first edition unchanged, as I apprehend it contains the germs of most of the ideas, which I have since expanded more at large in different Essays, and which have been considered as discoveries of some importance:—for instance, the idea that steam, or the vapour of water, is an independent elastic fluid, so largely insisted upon in the sixth essay: and hence, that all elastic fluids, whether alone or mixed, exist independently."

This work, when it first appeared in 1793, fairly represented the actual state of meteorological knowledge, and even contributed materially to advance that branch of science; but it cannot be pronounced a happy idea to reprint, after the lapse of forty years, unchanged and unexpanded, a work treating of so eminently progressive a department of science. All that can be said in palliation of such indifference to the rapid march of discovery is, that Dalton was, from temperament, averse to relinquish, or even modify, views adopted only after prolonged digestion, and assimilated into his inmost nature. He continued to observe with his old and somewhat rude instruments, and never received into favour the more rigorous methods or the more refined and exact apparatus of modern times. It cannot therefore be expedient to enter upon a minute analysis of this early work. The foreshadowings, which it casts, of some of his subsequent discoveries, will be more conveniently noticed when we review, in their chronological sequence, the special memoirs in which his new doctrines were fully developed and established.

The important section on the Aurora Borealis may, however, be now most conveniently noticed. In June, 1788, rather more than a year after he first commenced keeping a Meteorological Journal, he informs Mr. Crosthwaite that he had "added a fresh column relative to the tides of the air. What gave rise to this was a supposition that these tides

may possibly give birth to some of the more minute changes in the weather, or that they may have some influence on the Aurora Borealis, a phenomenon which has baffled the sagacity of the last and present age to account for in a satisfactory manner." Afterwards, in February, 1793, he tells his correspondent: "I am engaged at present in observing the *daily* variation of the needle by an excellent compass. The Aurora Borealis disturbs the needle pretty much, perhaps half a degree or more, during its action in the air. This was first discovered by an Italian philosopher; but I have discovered a further connection betwixt these two so apparently different phenomena of the Aurora Borealis and magnetism.* Instead of observing in future to what point the beams of light converge, observe at what *point of the compass* the beams rise *directly upwards* or perpendicular to the horizon." Again, in April of the same year, he writes to Mr. Crosthwaite: "It will be unnecessary to remark my very high satisfaction with thy observations on the Aurora. I think no one could have done better. I should wish to know whether the observation of the altitude was repeated or only taken once. Upon reviewing my observations, I find the altitude here was 53°; thine was 48°; the difference 5° gives the height about 150 miles. I think the true altitude here would not be 2° over or under; probably there the altitude would be within 2° of 48° also. The height of this arc must therefore be very great, and much higher than the atmosphere has usually been supposed. I should like to have at some opportunity the notes thou hast made upon the other Aurora this winter, and then I think thou may desist from so watchful and particular care of these phenomena, as we shall hardly have another opportunity so fine as that above, of determining their height."

It was on these synchronous observations, made February 15, 1793, by himself at Kendal, and by Mr. Crosthwaite at Keswick, stations about twenty-two miles apart, that his cal-

* Dr. Halley is quoted by Humboldt, "as having put forward as a bold conjecture, more than 130 years ago, that the Aurora is a magnetic phenomenon."—See Motte's *Abridgement of Phil. Trans.* vol. ii. p. 124, *et seq.*

culation of the height of the Aurora, 150 miles, was founded.* In the letter above quoted, and in Essay eighth on the Aurora Borealis, he advances his claim to "an original discovery, which seems to open a new field of inquiry in philosophy, or rather perhaps to extend the bounds of one that has been, as yet, but just opened." "The very grand Aurora in the evening of the 13th of October, 1792, was that which first suggested and led to the discovery of the relation betwixt the phenomenon and the earth's magnetism. When the theodolite was adjusted without doors, and the needle at rest, it was next to impossible not to notice the exactitude with which the needle pointed to the middle of the northern concentric arches; soon after, the grand dome being formed, it was divided so evidently into two similar parts, by the plane of the magnetic meridian,† that the circumstances seemed extremely improbable to be fortuitous, and a line drawn to the vertex of the dome, being in direction of the dipping needle, it followed, from what had been done before, that the luminous beams at that time were all parallel to the dipping-needle."

After demonstrating a series of "mathematical propositions necessary for illustrating and confirming those concerning the Aurora Borealis," he describes the phenomena, and enunciates six propositions concerning the Aurora. Of these, the two most important are, that "the cylindrical beams of the Aurora Borealis are all magnetic, and parallel to the dipping needle at the places over which they appear," and that "the height of the rainbow-like arches of the Aurora above the earth's surface is about 150 English miles." This last determination he afterwards found reason to modify in a memoir which was published in the *Philos. Transactions* for 1828, p. 291. A very remarkable Aurora had been

* *Met. Essays*, pp. 69 and 149.

† Humboldt however states: "The azimuth of the highest point of the luminous arch, when carefully measured, has been usually found not quite in the magnetic meridian, but from five to eighteen degrees from it, on the side towards which the magnetic declination of the place is directed."—Col. Sabine's *Cosmos*, vol. i. p. 181. Kämtz also quotes numerous observations to the same purport.—*Lehrbuch der Meteorologie*, B. 3, p. 452.

observed on the evening of March 29, 1826, at numerous points in Scotland and the North of England; and although unfortunately the Edinburgh and Keswick observations did not harmonize, yet calculations resting on those made at Whitehaven and Warrington,—stations 83 miles apart,—nearly concurred with others made at Jedburgh and Warrington in fixing the altitude of the auroral arch at from 100 to 110 miles. Dalton's memoir called forth one from the Rev. James Farquharson of Alford, which appeared in the *Phil. Trans.* for 1829. Mr. Farquharson observed that several successive arches of the Aurora often appear at the same time within the field of view; and contends that "the numerous observations, made on March 29, 1826, may be more easily explained and rendered consistent with each other on the supposition, that there were several nearly vertical fringes of the Aurora, almost contemporaneously hanging over many lines from Edinburgh to Warrington, *at a few thousand feet above the surface of the earth*; . . . that the region it occupies is above and contiguous to that of the clouds, or that in which they are about to form."

At the meeting of the British Association at Cambridge in 1833, an interesting discussion took place, in which Dalton participated, respecting the Aurora Borealis, and a committee was appointed, "in the express view of directing observers to the really important features of this meteor, and of obtaining, by a system of contemporaneous observation, data which experience shows cannot be derived from insulated exertion." The arguments which were advanced by several eminent men present, in support of a much lower altitude of the meteor than he had assigned, did not avail to modify Dalton's ideas. For, in the Appendix to his Meteorological Essays, published in the following year 1834, he examines Mr. Farquharson's objections and Sir E. Parry's observations, and concludes in maintaining the accuracy of his own calculation. It has not, however, been confirmed by subsequent observers. To M. Arago he thus wrote, May 4, 1836.—"You will probably have heard of the very fine Aurora Borealis, which was seen everywhere in Great

Britain and Ireland on the 17th and 18th of November last. It is remarkable that the same were seen on the same evenings over most of the United States of America; both in England and America the light was so great as to spread the alarm of fire in the country, and the firemen were called out. Vessels from America to England observed the same Auroræ on the Atlantic." I have found in his letter-book the following characteristic passage, in a letter addressed to Dr. Faraday, September 3, 1840.—" I observe the Council have voted the Rev. Mr. Farquharson's paper as fit for publication in the Second Part, 1839. The height of the Aurora was 1897 yards, or rather above 1 mile; I calculated it 100 to 160 miles (1828); Mr. Cavendish, 52 to 70 miles (1790); Robert Were Fox, 1000 miles (1831). Surely this would be an interesting phenomenon to the British Association, whether its height was 1 mile or 1000 miles." Humboldt concludes: " What we know of the height of the Aurora is grounded on measurements, which, from their nature, and from the incessant fluctuation of the phenomenon, and consequent uncertainty of the parallactic angle, cannot inspire much confidence. Without including older statements, the results of these measurements give heights varying from a few thousand feet to several miles.—*Cosmos,* p. 184. Humboldt also affirms that " every observer certainly sees his own Aurora as well as his own rainbow; but the phenomenon of the effusion of light is generated by a large portion of the earth at once." (p. 183.) Kämtz, after enumerating several discordant measurements, expresses distrust in the modes of determination.—*Lehrbuch,* B. 3. p. 472. Colonel Sabine thus replies to inquiries, made at my request by Dr. Daubeny, respecting his own view of the height of the Aurora.

Woolwich, Oct. 1, 1853.

" Dalton's opinions, regarding the height of the Aurora, were formed at a period when very much less attention had been paid to the phenomenon than has been the case subsequently. There are very good papers in the Philosophical Transactions, written by the late Mr. Farquharson, of

Alford, about 1829, and others later, showing its frequent occurrence at heights from 1500 to 2500 feet; and in our arctic expeditions, occasions were not uncommon when the Aurora (without the possibility of mistake,) was seen to rest on the surface of the sea or land. The connection between the appearance of Auroræ and disturbances of the terrestrial magnetism has been one of the subjects of inquiry at the magnetical and meteorological observatories, during the last ten or twelve years. The records of many of these observatories contain descriptions of the varying phases of the Aurora, with the exact time of their occurrence, and simultaneous records of the amount of magnetic disturbance, of the declinatic, inclinatic, and magnetic force; and, of late, a still higher interest has been imparted to the study of these concurrent phenomena, by the strong probability which has been established, that they are both intimately connected with the phenomena of the *solar spots,* and that all these phenomena are subject alike to a periodical inequality, extending for rather more than ten of our years, with a maximum and a minimum separated from each other by an interval somewhat exceeding five of our years. I mention these matters in evidence, that the subject has not *stood still* since Dalton's time. . . . . Arago's opinion, that "every one sees his own Aurora," and "no two men the same," has, so far as I know, never been successfully controverted, and seems to be at variance with a principle on which Dalton's deduction, as to the height of the phenomenon, was founded." Again, in a note to myself, November 11, Colonel Sabine writes: "In the notes to *Cosmos,* Vol. i. p. xxxiv, I have recorded an instance, which fell under my own observation in Skye, of an Aurora, of similar character to those described by Mr. Farquharson, low in the atmosphere, having during the day the appearance of a thin mist, permitting the form of the hill, and the irregularities of the surface of the ground, to be distinctly visible through it, and at night becoming luminous with auroral streamers proceeding from it."

Dalton became in 1794 a member of the Literary and Philosophical Society of Manchester, one of the earliest, and

not the least eminent, of the provincial societies that have been formed for the culture of science and letters. He was soon after chosen a member of the Committee of Papers, and filled in succession all its offices of honour till he attained, in 1817, its highest dignity. He continued to occupy the President's chair during the remainder of his life, and at his decease was succeeded by the late Dr. Holme, also a native of Kendal. The Society, with a wise liberality, permitted him to occupy one of the lower rooms, in their house in George-street, as a study and laboratory. In this small apartment the larger portion of his subsequent life was passed, in the labour of private tuition, and in the prosecution of experimental research. It was therefore not an unfitting return to the Society, that he published in their memoirs the long series of important essays, in which his successive discoveries were first made known.

His earliest communication, entitled, "Extraordinary Facts relating to the Vision of Colours, with Observations," was read before the Society, October 31, 1794, and is published, Vol. v. p. 28. He observes, "Since the year 1790, the occasional study of botany obliged me to attend more to colours than before. With respect to colours that were white, yellow, or green, I readily assented to the appropriate term; blue, purple, pink, and crimson appeared rather less distinguishable, being, according to my idea, all referable to blue. I have often seriously asked a person whether a flower was blue or pink, but was generally considered to be in jest." He ascertained on inquiry that his brother, and several other persons, possessed the same peculiarity of vision. He thus sums up the characteristic facts.

1. "In the solar spectrum three colours appear, yellow, blue, and purple. The two former make a contrast, the two latter seem to differ more in degree than in kind.

2. "Pink appears by daylight to be sky-blue, a little faded; by candlelight it assumes an orange or yellowish appearance, which forms a strong contrast to blue.

3. "Crimson appears a muddy blue by day, and crimson woollen-yarn is much the same as dark blue.

4. "Red and scarlet have a more vivid and flaming appearance by candle-light than by day-light."

He concludes: "It appears therefore, almost beyond a doubt, that one of the humours of my eye, and of the eyes of my fellows, is a coloured medium, probably some modification of blue. I suppose it must be the vitreous humour, otherwise I apprehend it might be discovered by inspection, which has not been done."

This theory was not, however, verified by an examination of the eye, which was carefully made after Dalton's death, in conformity with his strongly expressed wish, by his skilful medical attendant, Mr. Ransome. The vitreous humour was of a pale yellow colour, and when used as a lens, it caused no modification of tint in red and green objects. Among Dalton's papers had fortunately been preserved the following important letter from Sir John Herschel, which he has kindly permitted me to use, observing, "It is true that there are some things in it, which I should not now consider free from objection, but they are not of such a nature as to make it worth while to alter or erase them."

"Slough, May 20, 1833.

"My dear Sir,—Nothing but the pressure of a work, which I have been preparing for immediate publication, and which I have at length got off my hands, no longer ago than this morning, would have prevented so long my replying to your most obliging letter, and thanking you for the valuable information contained in your replies to my optical queries. They agree, on the whole, with the views I had taken of this singular affection of vision, and seem to throw much light on the matter.

"It is clear to me that you and all others so affected perceive *as light* every ray, which others do. The retina *is excited* by every ray which reaches it, nor, so far as I can see, is there the slightest ground for believing that any ray is prevented from reaching it by the media of the eye. This, I know, is contrary to your opinion, but I speak advisedly and after careful consideration. Yourself and all other persons whose vision is of this class Whewell calls *Idiopts*.

(I don't like the word; it is too like *idiots*). Call them, however, what we may, it is evident they *see* by rays as strictly homogeneous as prismatic or absorptive analysis can make them, whether *we* should call those rays red, blue, yellow, or any other names. This is one important step gained, and this your replies to my queries fully prove.

"The question, then, is reduced to one of pure sensation. It seems to me, that we have three primary sensations when you have only two. We refer or can refer in imagination, all colours to three, red, yellow, blue. All other colours we think we perceive to be mixtures of these, and can produce them by actual mixture of powders of these hues; whereas we cannot produce these hues by any mixture of others. Thus green is yellow + blue, orange is yellow + red, purple is blue + red, brown is black + red or black + orange or black + yellow, as the case may be; it is essentially sombre, and depends for its effect *as brown* entirely on the proximity and contrast of bright hues. Now to eyes of your kind it seems to me that all your tints are referable to two, which I shall call A and B; the equilibrium of A and B producing *your* white, their negation your black, and their mixture in various proportions your compound tints. With regard to what sort of sensations A and B are, of course we can no more tell than you can tell what our α, β, and γ (red, yellow, and blue) are.

"Only this appears to me demonstrated by all the cross-examinations I have ever been able to give any persons so affected with what I think, after all, may be termed 'Dichromic Vision,'—as well as by your answers to my queries,—viz. that the same rays which excite in us the sensation γ (blue), excite in you the sensation B; and that rays which excite in us the two distinct sensations α and β, excite in you only the one sensation A. I will not be positive that it is always so strictly definite. I think, on the contrary, that there are occasional, though rare, cases of imperfect dichromatism, where an obscure perception of *some difference* between α and β exists, without amounting distinctly to a conviction that it is anything more than that the one is rather a harsher sort of the other.

"I should like much to know whether you have ever attempted to reduce all sensations of colour to a mixture of certain primary sensations, as Mayer has done, into red, yellow, and blue; and with what success, or whether you have time and inclination to make the attempt. I speak with reference to no theory of light or of the constitution of the spectrum, which does not seem to me to touch the subject any more than questions about *bitter, sweet,* or *salt,* can be resolved by talking of the chemical analysis of the things tasted."*

An interval of nearly five years elapsed before Dalton made his next communication to the Philosophical Society, entitled, "Experiments and Observations to determine, whether the quantity of rain and dew is equal to the quantity of water carried off by the rivers, and raised by evaporation: with an Inquiry into the origin of Springs."—Read March 1, 1799. He concludes in favour of their equiponderance; but the experiments were not from their nature susceptible of accuracy, and the reasonings do not amount to anything like rigorous induction. The most valuable part of this essay is to be found in a note (p. 351) which contains his first distinct enunciation of the theory of aqueous vapour.

"1. That aqueous vapour is an elastic fluid *sui generis,* diffusible in the atmosphere, but forming no chemical combination with it.

"2. That temperature alone limits the maximum of vapour in the atmosphere.

"3. That there exists at all times, and in all places, a quantity of aqueous vapour in the atmosphere, variable according to circumstances."

This essay is immediately followed by one "On the Power of Fluids to conduct Heat," suggested by Count Rumford's seventh essay. The most interesting portion of this paper is the preliminary inquiry into the maximum density of water. I find the first mention of these experi-

* See the Appendix on Colour-Blindness, by Dr. George Wilson.

ments in a letter addressed to his brother, March 28, 1799. "I have lately been making some curious experiments on the congelation of water in certain circumstances. I have cooled it down to 5° or 6° without freezing, by putting it into a thermometer tube. I find it almost impracticable to freeze it in such circumstances above 15° or 20°; when it does freeze, it is instantaneous, and the liquor shoots up the tube, as if ejected by a syringe, and often bursts the tube with a report. The degree of greatest condensation in water is, however, at 42°; that temperature giving the lowest point in the scale of a water thermometer. The most remarkable fact, however, and one which has never, that I know of, been ascertained before, is that water expands below 42° exactly as it does above, namely, according to the number of degrees; thus water of 32° is of equal density with water of 52° and water of 6° equal to water of 78°, &c.; but the moment it freezes, its dimensions are increased." In his printed essay, read April 12, 1799, he assigns 42½° as the temperature of maximum density. He afterwards corrected all these numerical results. Thus he found that the expansions produced by decrements of heat from the point of greatest density are *greater* than those caused by the same increments of heat. In an experiment with a water thermometer, the fluid stood at the same point at 19° and 66½° Hence a decrement of 23° (42° − 19°) produced the same expansion as an increment of 24½° (66½ − 42°).* This difference might indeed have been anticipated from the sudden and vast expansion that accompanies the act of congelation. For it would seem probable, that the particles of water must be slowly approaching that polar arrangement, constituting the state of congelation or crystallization, at temperatures approximating 32° and, *a fortiori*, at those considerably below that degree.

* This experiment was communicated to me, at that time his pupil, March 3, 1823. He then regarded 42°, not 42½°, as the apparent maximum density of water in a *glass* vessel. (*New System*, vol. i. p. 31.) If 40° represent the apparent greatest density in glass, 21° decrement would be equivalent to 26½° increment.

It is important to observe that in this earliest notice the necessary correction for the expansion of the glass vessel had not occurred to his mind. He certainly nowhere in the course of the essay draws any distinction between the *real* and *apparent* point of greatest density. In *Nicholson's Journal* are Essays by him, called forth by Dr. Hope's beautiful experiments. He also recurs to the subject in his *New System*, vol. i. (pp. 23 and 30), and concludes that the *real* greatest density is at the temperature of 36°. Afterwards the eminently precise experiments of Petit and Dulong on the expansions of glass and other solids supplied more trustworthy data for calculation; and he informed me in 1823, that he regarded 38° as the true point of greatest condensation of water. But the concurring testimony of recent experimenters places it higher. We may accept Messrs. Playfair and Joule's determination of 39·101°, confirmed by the almost identical numbers of Gay-Lussac and Hällstrom (39°) as deserving of entire confidence.*

The main design of the Memoir was to refute experimentally, Count Rumford's conclusion, "that water, and by analogy, all other fluids, do not transmit heat in the manner that solids do, but circulate it solely by the internal motion of their particles."

Mr. Dalton infers from numerous experiments that water has a proper conducting power. It is, however, equally evident that water is a bad conductor of heat. Mercury, in his seventh experiment, appeared to conduct much more decidedly. Ice proved a worse conductor than water. His conclusion is probably correct, being confirmed by subsequent experiments. But it must be owned that his own were not sufficiently accurate to determine so delicate a question. The very minute transmission of heat downwards, which he observed, might be referred to the accidental, scarcely avoidable commingling of the liquids of different temperatures, in the act of pouring, or to conduction by the sides of the containing vessel. Dalton never possessed the

* Gmelin's *Handbook* (Cav. Soc.), vol. i. p. 225.

refined instruments, nor had he the manual dexterity, or the mental habitudes or temperament, essential to rigorous experimental determinations.

In the same volume (p. 515) is a beautiful memoir "On the Heat and Cold produced by the Mechanical Condensation and Rarefaction of Air." It had been long known that the condensation and rarefaction of air produced a sensible change in its temperature, the former occasioning a *sudden* rise, and the latter a *sudden* depression of the thermometer; but before Dalton's experiments, the amount of these changes had been vastly under-estimated. He showed the inadequacy of the mercurial thermometer to serve as a measure of this evanescent elevation of temperature, and by a simple and most ingenious contrivance, obtained a much closer approximation to the heat evolved or absorbed. He adopts the theory of Lambert, that a vacuum contains an absolute quantity of heat, having its proper capacity for heat; and this heat, when added to the caloric introduced by the entering air, necessarily produces an accumulation of heat and a consequent elevation of temperature. He concludes "we may hence be led into a train of experiments, by which the absolute capacity of a vacuum for heat may be determined, and likewise the capacities of the different gases for heat, by a method wholly new; but this must be left to future investigation." It is worthy of remark, that this method was actually employed to measure the specific heat of the gases by MM. Desormes and Clement in 1819, and subsequently by MM. De la Rive and Marcet. Dalton always regarded this memoir with marked paternal predilection, and caused the condensing pump to be introduced, together with a paper of atomic symbols, into the back-ground of his first portrait by Allen.

His next contribution to the Manchester Memoirs, read October 2, 16, and 30, 1801, (vol. v. p. 535) is beyond comparison the most original and important of his experimental works. It is entitled "Experimental Essays on the Constitution of Mixed Gases; on the force of Steam or Vapour from Water and other Liquids in different temperatures,

both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expansion of Gases by Heat." He manifestly writes with a deep but not undue sense of the importance of the novel views he is about to disclose; and his style, usually concise and simple even to dryness, becomes fuller and more elevated. "The progress of philosophical knowledge," he commences, "is advanced by the discovery of new and important facts; but much more when those facts lead to the establishment of *general* laws. . . . In the train of experiments lately engaging my attention, some new facts have been ascertained, which, with others, seem to authorize the deduction of general laws, and such as will have influence in various departments of natural philosophy and chemistry." He has also departed from the method observed by most scientific inquirers, by propounding these general laws, before describing the experiments from which they were derived. But while he, in this instance, prefers the synthetical to the analytical form of exposition, he carefully guards against the suspicion of having devised the experiments to establish a pre-conceived hypothesis. "On the contrary, the first law, which is as a mirror, in which all the experiments are best viewed, was *last* detected, and after all the particular facts had been previously ascertained."

"1. When two elastic fluids, denoted by A and B, are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of A do not repel those of B, as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind.

"2. The force of steam from all liquids is the same, at equal distances above or below the several temperatures, at which they boil in the open air: and that force is the same, under any pressure of another elastic fluid, as it is in vacuo. . . . .

"3. The quantity of any liquid evaporated in the open air, is directly as the force of steam from such liquid at its temperature, all the circumstances being the same.

"4. All elastic fluids expand the same quantity by heat;

and this expansion is very nearly in the same equable way as that of mercury, at least from 32° to 212°. It seems probable the expansion of each particle of the same fluid, or its sphere of influence, is directly as the quantity of heat combined with it; and consequently the expansion of the fluid as the cube of the temperature, reckoned from the point of total privation."

Essay I. "On the constitution of Mixed Gases; and particularly of the Atmosphere."

Since the discovery of the compound nature of the atmosphere, two opposite theories of its constitution had been proposed; the one, especially enforced by Berthollet, that it was a true though unstable chemical combination of oxygen and nitrogen; the other, that it was merely a mechanical mixture of those gases. After contemplating all possible mutual relations of the particles of mixed elastic fluids, Dalton maintains that the phenomena best accord with the hypothesis that "the particles of one elastic fluid may possess no repulsive or attractive power, or be perfectly inelastic with regard to the particles of another; and consequently, the mutual action of the fluids be subject to the laws of inelastic bodies." According to this hypothesis, if m measures of A be mixed with n measures of B, the two will occupy $m+n$ measures of space. The particles of A meeting with no repulsion from those of B, further than that repulsion which, as obstacles in the way, they may exert, would instantaneously recede from each other as far as possible in their circumstances, and consequently arrange themselves just the same as in a void space."

This doctrine did not command the assent of chemists. It was at once assailed by Berthollet, Gough, Thomson, and Murray; and among the objections raised against it, some were regarded by Dalton himself as sufficiently cogent to induce him, when treating the same subject in his *New System* (p. 189), to admit that "the phenomena of mixed gases may still be accounted for by repulsion, without the postulatum that their particles are mutually inelastic." The whole history of this controversy, and the full exposition of

Dalton's modified theory, are to be found in his *New System of Chemical Philosophy*, Part i., pp. 150—193, and need not be here reproduced. I have elsewhere argued,* that if heat be the sole cause of repulsion, as is implied in Laplace's theory of elastic fluidity, it is impossible to admit that the atmosphere of heat surrounding the atoms of any gas A, and constituting those atoms mutually repulsive, can be indifferent to portions of the same heat associated with the atoms of another gas B. Laplace, in applying the analytical calculus to the condition of mixed gases, and of gases mixed with vapours, rejects the supposed absence of repulsion between the molecules of different gases as theoretically improbable, and as inconsistent with known phenomena.† In reviewing a subsequent essay by Dalton, it will also be shown, that there is one undoubted experimental truth scarcely to be reconciled with his original hypothesis.

Essay II. "On the force of steam or vapour from water, and various other liquids, both in a vacuum and in air."

This essay is one of transcendent importance, as first furnishing tabulated data for the solution of perhaps the most interesting problem in meteorology; viz., the calculation, after noting the dew-point, of the absolute quantity of moisture in a given volume of air. It opens with the following anticipation of a subsequent discovery by Mr. Faraday:—"There can scarcely be a doubt entertained respecting the reducibility of all elastic fluids, of whatever kind, into liquids; and we ought not to despair of effecting it in low temperatures, and by strong pressure exerted upon the unmixed gases." He had previously, in his *Meteorological Essays* (p. 127, 2nd ed.), published a table of the temperatures at which water boils, and the corresponding pressures, from 80° to 212°. He now ascertains, by subjecting a small portion of water, floating on the summit of the column of mer-

* In an essay read before the Manchester Society, and in Dalton's presence, previously submitted to him in manuscript, and, as far as I can remember, sanctioned by his approval.—*Phil. Magazine*, May, 1835.

† Cette hypothèse est bien peu naturelle, elle est d'ailleurs contraire à plusieurs phénomènes.—*Méc. Céleste*, tome v. p. 109 and 110, liv. xii, § 5.

cury in a barometrical tube, to various artificial temperatures, the force of aqueous vapour at different degrees between 32°, and 212°. "Upon examination of the numbers in the table within the limits just mentioned, there appears something like a geometrical progression in the forces of vapour; the ratio, however, instead of being constant, is a gradually diminishing one." On this principle, he ventures to extend the table by calculation both above and below the limits of his experiments; as probably furnishing "a near approximation." His experimental results were confirmed, with slight deviations, by Dr. Ure; but the numbers calculated for higher temperatures depart considerably from those obtained by subsequent inquirers, and especially by MM. Dulong and Arago. He himself, at a later period, 1819, detected their inaccuracy. "I have lately made, for the first time, various experiments on the force of steam from water, in temperatures from 212° to 300°; the results of which convince me that the theoretic forces which I gave in the fifth volume of the *Memoirs*, as also those subsequently in my *Chemistry*, are both erroneous; the former being about as much too small as the latter are too large, so that the mean of the two series is a near approximation to the truth." (*Manchester Memoirs, N. S.*, vol. iii., p. 471.) Even his experimental numbers must now be regarded as superseded by the precise results of Regnault and Magnus.

He proceeds to institute similar experiments, with certain necessary modifications of the apparatus, on the vapours of sulphuric ether, spirit of wine, water of ammonia, solution of muriate of lime, mercury, and sulphuric acid. "Experiments made upon these six different liquids agree in establishing this as a general law; namely, *that the variation of the force of vapour from all liquids is the same for the same variation of temperature, reckoning from vapour of any given force:* thus, assuming a force equal to 30 inches of mercury as the standard, it being the force of vapour from any liquid boiling in the open air, we find aqueous vapour loses half its force by a diminution of 30° of temperature; so does the vapour of any other liquid lose half its force by diminishing its tem-

perature 30° below that in which it boils, and the like for any other increment or decrement of heat." It was perhaps not a hasty, though doubtless not a rigid induction from the similar habitudes of *six* liquids, differing so widely from each other as those above enumerated, that *all* liquids would obey the same law. Exceptions, as rock oil and oil of turpentine, have been subsequently ascertained; and in cases where the law does seem to obtain, the intervals of temperature, characterized by equal forces of vapour, are not precisely identical. "It is nevertheless remarkable," observes Gmelin,* that this law is pretty nearly true in the case of many substances." Dove's comparison of the tensions of permanent gases, as determined by Faraday, with that of vapour of water, as ascertained by Arago and Dulong, likewise speaks in favour of this law. We are therefore entitled to regard this beautiful relation, though not of universal compass, yet (like that of isomorphism, similarly limited by apparent exceptions)† as distinctly pointing to some simple, comprehensive law of nature, to be revealed by future investigation.

With reference to vapour in air, Dalton thus expresses himself:—

"It will be unnecessary to repeat in detail the numerous experiments made on the various liquids in all temperatures from 32° to 212°; as the results of all agree in one general rule or principle, which is this: let 1 represent the space occupied by any kind of air of a given temperature, and free from moisture; p = the given pressure upon it in inches of mercury; f = the force of vapour from any liquid in that temperature, in vacuo; then the liquid being admitted to the air, an expansion ensues, and the space occupied by the air becomes immediately, or in a short time $= 1\ \frac{f}{p-f}$, or $= \frac{p}{p-f}$." He concludes: "The notion of a chemical affinity subsisting between the gases and vapours of different kinds cannot at all be reconciled to these phenomena. To suppose that all the different gases have the same affinity for water might indeed

* Gmelin's *Handbook* (Cavendish Society) vol. i. p. 265.

† Otto, in *Chemical Reports* (Cavendish Society) vol. i. p. 129.

be admitted, if we could not explain the phenomena without it; but to go further, and suppose that water combines with every gas to the same amount as its vapour in vacuo, or, in other words, that the elasticity of the compound should be exactly the same as if the two were separate, is certainly going far to serve an hypothesis."

Essay III. "On Evaporation." The comparative amounts of loss from evaporation of water at temperatures between 212° and 138° were first ascertained. It is impossible to claim for these experiments the merit of being more than rude and remote approximations to truth. The greater or less intensity of the source of heat, in the case of loss at the temperature of ebullition, and the state of the surrounding air as respects calmness or motion, at all temperatures induced large variations in the numbers obtained. Thus at 212° the extremes were 30 and 45 grains per minute; at 180°, 18 to 22 grains; at 164, 10 to 16 grains. On comparing, however, the average results, at each temperature, with the forces of vapour of the same temperature, he was agreeably surprised to find that they exactly corresponded in every part of the thermometric scale. At lower temperatures than 138° he found that regard must be had to the force of vapour actually existing in the atmosphere at the time; the effective evaporating force being universally equal to that of the temperature of the water, diminished by that already existing in the atmosphere. This latter force is measured by ascertaining the temperature at which dew begins to be deposited, either by the simple process of Leroy, adopted by Dalton, or by the elegant hygrometers of Daniell and Regnault.

Essay IV. "On the Expansion of elastic Fluids by Heat." This inquiry was suggested by a memoir by MM. de Morveau and DuVernois, who had disputed the concurrent testimony of De Luc, Roi, Saussure, and Berthollet. Dalton ascertained by repeated experiments that 1000 parts of common air of the temperature 55° and common pressure, expand to 1325 parts, when heated to the temperature of 212°. He also found that hydrogen, oxygen, carbonic acid

gas, and nitrous gas, expand to the same amount as common air; the minute differences observed being attributable to the presence of aqueous vapour. Gay-Lussac obtained in the same year (1801) results differing but slightly from those of Dalton, the expansion for a single degree of Fahrenheit being, according to Gay Lussac, $\frac{1}{480}$ of the primitive volume at 32°, and according to Dalton $\frac{1}{483}$. Magnus and Regnault, by recent and most exact experiments, have determined the expansion to be $\frac{1}{491}$. They have also shown that the coefficient of expansion is not precisely the same for all gases; the more liquefiable gases being more expansible by heat, as might indeed have been anticipated from their being more condensible by pressure than the permanent gases.

From these experiments Dalton is disposed to conclude "that all elastic fluids, under the same pressure, expand equally by heat, and that for any given expansion of mercury the corresponding expansion of air is proportionally something less, the higher the temperature. . . . It seems, therefore, that general laws respecting the absolute quantity and the nature of heat are more likely to be derived from elastic fluids than from other substances. . . . As every other liquid we are acquainted with is found to expand more in the higher than in the lower temperatures, analogy is in favour of the conclusions of De Luc, that mercury does the same." It is scarcely possible to over-estimate the value of these sagacious conclusions. They may be affirmed to lie at the basis of the profound and hitherto unrivalled memoir, by MM. Dulong and Petit, on the measure of temperature. In order to constitute any body a correct admeasurer of temperature, it is obvious that equal increments of heat should, in all parts of the scale, be followed by equal degrees of expansion. This condition they conceive to be *most probably* answered in all temperatures to which we have access, by the gases, which expand equally $\frac{1}{483}$ of their bulk at 32° for every degree of Fahr. The air thermometer, corrected for the unequal expansions of glass, was therefore the standard, to which MM. Dulong and Petit referred the dilatations of mercury

and all the other bodies that they subjected to experiment. It is well known that their singularly precise experiments signally confirmed Dalton's sagacious inferences from his less exact researches. Mercury and all other solid and liquid bodies exhibit a variable expansion, when compared with that of air, an expansion greater at higher than at lower temperatures. Thus the corrected mercurial thermometer would mark a temperature of 204·6° Cent., when the air thermometer indicates 200° Cent. and 314·15° at 300° of the air thermometer. Even at temperatures between 32° and 212° Fahr., in which interval Dulong and Petit believed that the air and mercurial thermometers expand equally, I was informed by that emphatically exact observer, the late Dr. Prout, that he had detected notable differences between the expansions of air and mercury; differences which necessarily attain their maximum at a station equidistant between the freezing and boiling points. These experiments are still inedited. Dalton, with his usual sagacity, had anticipated such results. In the appendix to his second volume, p. 289, he observes: "Like the scales of air and mercury, which are so nearly coincident from −40° to 212° that scarcely any difference is sensible, *though no one doubts of its existence.*"

It has seemed expedient to dwell on these four remarkable essays more amply than on those which succeed, because, independently of their momentous bearing on meteorological science, they are deeply stamped with the impress of Dalton's genius, and furnish instructive types of his modes of working and thinking. His instruments of research, chiefly made by his own hands, were incapable of affording accurate results, and his manner of experimenting was loose, if not slovenly. His numerical determinations have not, therefore, like even the earlier analyses of Prout, been confirmed by subsequent inquirers. Still, his experiments, though wanting the exactitude of modern research, were not unskilfully devised and were most sagaciously interpreted. They were, perhaps, such as were most needed at the close of the last century, when so many fields of experimental research were untilled, that bold tenta-

tive incursions into new domains of thought, large groupings, and happy generalizations of approximative results were more effective instruments of advance than scrupulous precision in details. At all events, from these imperfect experiments, Dalton arrived at the discovery of those general laws of evaporation and of the relations of air with moisture, which were translated by Biot into the exact language of analytical formulæ, and which still constitute the foundation of meteorological science.

CHAPTER III.

UNEVENTFUL LIFE—EXTRACTS FROM LETTERS TO HIS BROTHER — ELEMENTS OF ENGLISH GRAMMAR—VISITS TO LANCASTER AND THE LAKES—LECTURES AT THE ROYAL INSTITUTION, 1804—EXPERIMENTS ON ATMOSPHERIC AIR—LAW OF MULTIPLE PROPORTIONS—SUBSEQUENT MEMOIRS ON THE ATMOSPHERE, 1826 AND 1837—DIFFUSION OF GASES—ABSORPTION OF GASES BY WATER —FIRST TABLE OF ATOMIC WEIGHTS, 1803—GENERAL SURVEY OF THE FIRST DECENNIAL PERIOD OF HIS LIFE IN MANCHESTER—FIRST PART OF NEW SYSTEM OF CHEMICAL PHILOSOPHY PUBLISHED, 1808 — LAWS OF TEMPERATURE, REFUTED BY PETIT AND DULONG, AND FINALLY ABANDONED BY DALTON—SPECIFIC HEATS OF ATOMS—FORESHADOWINGS OF THE ATOMIC THEORY.

The lives of men, earnestly and exclusively devoted to the pursuit of science, present few incidents calculated to awaken general interest. That of Dalton was pre-eminently unvaried and uneventful. His biographer has little more to record than the successive births of the children of his intellect. "Il a été assez heureux ou assez sage, pourque l'on ne sache presqu'autre chose de lui, et qu'il n'y ait dans son histoire d'autres incidens que des découvertes."* Some few particulars may, however, be gathered from his private journals and from his correspondence with his brother at Kendal, which may vary the monotony of an unbroken narrative of discoveries, and afford some glimpses of his social and moral qualities. But it is needful to confess, *in limine*, that his nature was not many-sided. His mental tendencies were exclusively meditative and ratiocinative; these impelled him to contemplate, with calm concentrated thought, the abstract relations of space and number. Thus the pure and mixed mathematics, and in natural philosophy, and even in chemistry, those departments most nearly allied to mathematics, and those

* Cuvier, *Eloge de Cavendish.*

relations most abstract and comprehensive, were, throughout life, the objects of his predilection. Botany was the only one among the sciences of classification, which he had cultivated with interest. Neither his early training nor his natural tastes had imbued him with a love of elegant letters. He had none of that ardent poetical temperament, that keen prophetic vision, which inspired the grand discoveries of Davy and of Faraday; nor that richly-varied mental culture, that refined taste, that profound sensibility, which charm us in the writings of Cuvier and Humboldt. There prevail, indeed, throughout his letters and his private journals, these last chiefly descriptive of tours in the North of England, a certain dryness and quaintness of style and manner, that render it difficult to select passages of much general interest. It is right, however, to let Dalton manifest himself, as he was and as he lived, in the strength and simplicity of his nature.

In May, 1792, he writes to his brother from London, then first visited by him, after expressing his amazement at the number and noise of the hackney coaches: "In short, this is a most suprising place, and worth one's while to see once; but the most disagreeable place on earth for one of a *contemplative turn* to reside in constantly." His numerical habitudes here incite him to count the number of coaches he meets in going to the Friends' morning meeting, a task "which he effected with tolerable precision; the number was 104." It may be well to preserve the following extract from a letter dated May, 1794, as a proof, if such were needed, of the humaner feelings and practices of the period in which we live:—"Having two or three vacant days at Easter, and the weather being fine, I determined to go into the country: accordingly, on seventh-day afternoon, I walked to Warrington, 18 miles. Nothing material occurred on the road till near entering the town, when I was struck with a sight I never beheld before, and which I did not know of: namely, a man gibbetted by the road-side, on a post as high as a steeple, his hat on his head and shoes on his feet, and completely dressed; it is a wonder that so shocking a spectacle, which everybody exclaims against, should still be continued

in any civilized country." As a favourable specimen of his descriptive manner may be extracted from his journal, August 22, 1796, the following passage:—"We had a pleasant ride from Kendal, for 8 miles, when the grand scenery of the lakes opened upon us with full force: the head of Winandermere and about half of the lake, with the surrounding hills, skirted with wood, formed a fine and capacious amphitheatre, which we had in view, more or less, till we arrived at Low Wood. Drank tea there, and immediately after took a boat out to a central part of the lake, when we beheld the sun descending below the summit of Langdale Pikes, whilst its rays still continued to gild the delightful landscape on the opposite shore. . . . . Came off the lake; then proceeded to Ambleside, winding round the still lake by twilight. . . . . Went out about ten to view the night scene: the atmosphere was as clear as possible; Jupiter and the fixed stars shone with uncommon splendour, and suggested an unusual proximity. The moon, risen, but not above the mountains, cast a glimmering light upon the rocky hills just opposite, and produced a fine effect. These circumstances, together with the awful silence around, would have persuaded us we had been transferred to some other planet."

These journeys to his native mountains, and occasionally to other parts of England, occupied agreeably and healthfully his annual vacations from the college, and subsequently from private tuition. It was his practice to perform them, in great measure, on foot. His muscular frame was well-developed, and he had brought from his native North a hardy and robust constitution, and an habitual need of active exercise. His time was so engrossed by his duties as a teacher, and by original labours in science, that, excepting one afternoon weekly, which he spent in the healthful game of bowls, in the country, he rarely allowed himself any relaxation. Thus he writes to his brother, Dec. 15, 1797:—"My time at present is much taken up with tuition at home and in the town together; so that I can scarcely turn to any particular mathematical or philosophical pursuit; but occasionally of

late I have been attending to the philosophy of Grammar and to that of Sound." Again in 1801, he writes :—"Since the year came in I have not been much troubled with *l'ennui*. Eight regular pupils by day, and as many more in the evenings, to whom I have sometimes given fifteen lessons a week;—my grammar in the press,—the whole of it to write over and to retouch, and to attend to the press;—have required a considerable activity both of body and mind."

The Elements of English Grammar, to which he here alludes, was published in 1801, and was dedicated to Horne Tooke, towards whom, in the Preface, he thus acknowledges his obligations :—"To the literary world it will be unnecessary to observe, that in this department, Etymology, I have drawn a great deal from *one source;* but I have not rested satisfied with the *ipse dixit* of the author of *The Diversions of Purley*, when time and opportunity afforded me means of confirmation and inquiry." He writes to his brother :—"I have sent one to J. Horne Tooke, but he has got *things* to attend to now, instead of *words*."* This little grammar is not destitute of merit, especially as bringing the *clara reperta* of Tooke, in a popular form, before a larger circle of readers. Dalton banishes the Articles from the Parts of Speech, and associates them with the Adjectives, under the title of Definitives. He has committed one error, rather surprising in a cultivator of the physical sciences. Among the "three ways of distinguishing the sexes in the English language," he mentions "change of termination, as—prince, princess—phenomenon, phenomena."

The philosophy of Sound he also alludes to as occupying much of his time. He desires his brother (April 14, 1797), "to remember him to John Gough, and let him know I have been reading Euler, Bernouilli, &c. on Sound. They have written very largely on the subject, chiefly in the *Petersburgh Transactions;* I think there are twenty or thirty papers by them and D'Alembert, from the years 1740—90. I find no English author any way comparable to them on

* Horne Tooke had at this time been arraigned on a charge of high treason.

this head." He probably engaged in this course of study, from its bearing on his Theory of Mixed Gases.

There occur numerous indications, in his letters and journals of this period, that Dalton, unlike Cavendish, but like most men of higher sensibility and intelligence, greatly enjoyed the society of women of superior talents and mental culture. There can surely now be no indiscretion, and it seems indispensable to an entire and faithful portraiture of his inmost heart and nature, to state, that on his numerous northern journeys there was one family of the Society of Friends residing in Lancaster, which he always revisited with intense pleasure. One of the daughters of the house seems, from the enthusiastic terms in which he speaks of her, to have been gifted with remarkable personal beauty, and general superiority of mind and character. He thus describes her to his brother, September 15, 1796:—"May here observe that it has been my lot for three years past to be daily gaining acquaintance of both sexes. I have consequently had opportunities of estimating and comparing characters upon a pretty extensive scale. Since my first introduction to —— twelve months ago, I have spent a day or two with them at six different intervals, with the highest satisfaction, as I never met with a character so finished as Hannah's. What is called strength of mind and sound judgment she possesses in a very eminent degree, with the rare coincidence of a quick apprehension and most lively imagination. Of sensibility she has a full share, but does not affectedly show it on every trivial occasion. The sick and poor of all descriptions are her personal care. Though undoubtedly accustomed to grave and serious reflections, all pensiveness and melancholy are banished from her presence, and nothing but cheerfulness and hilarity diffused around. Her uncommon natural abilities have been improved by cultivation; but art and form do not appear at all in her manner,—all is free, open, and unaffected. Extremely affable to all, though every one sees and acknowledges her superiority, no one can charge her with pride. She is, as might be expected, well pleased with the conversation of literary and scientific people,

and has herself produced some essays that would do credit to the first geniuses of the age, though they are scarcely known out of the family, so little is her vanity. Her person is agreeable, active, and lively. She supports conversation, whether serious, argumentative, or jocular, with uncommon address. In short, the *tout ensemble* is the most complete I ever beheld. Next to Hannah, her sister Ann takes it in my eye before all others. She is a perfect model of personal beauty; do not know one that will bear a comparison with her in this respect, at least in our Society. With abilities much superior to the generality, she possesses the most refined sensibility, but in strength of mind and vigour of understanding must yield to her elder sister. I dwell with pleasure upon the character of these two amiable creatures, but would not have thee communicate my sentiments to others." In the journal of a tour in the previous year, when he first became acquainted with this lady, he thus describes a walk in her company up the river Lune as far as Horton: —"The pleasantness of the evening, the delightful scenery of the country, added to the amiable softness, vivacity, and good sense of our female companion, made it one of the pleasantest walks I ever enjoyed." And again he quaintly remarks:—"In going to a tea-party we were introduced by our fair companion to the hospital for old maids, and saw one of no very alluring aspect. Oh, what a contrast!" There is no evidence before me, that this warm admiration ever manifested itself in a formal declaration. Dalton was not at that time in circumstances that would allow of his marrying; and remained, like his elder brother and sister, all his life unmarried. Forty years after, he visited the lady and her husband, on the occasion of one of the meetings of the British Association. His early journals show that his excited feelings often, at this time, sought expression in verse; and in the letter from which I have so largely extracted, are some "Stanzas addressed to an Eolian Lyre," which do not soar beyond respectable mediocrity. They are, however, here introduced as the most favourable specimen of his compositions in verse.

Stanzas addressed to an Eolian Lyre.

Far from the noisy dissonance of strife,
From war's dire clarion, boding vengeful ire,
Here let me spend one vacant hour of life,
To sing thy well-earned praise, melodious Lyre!

When thy soft airs first touch'd my ravish'd ear,
My heart accorded to the tender strain;
Now gently swelling, call'd forth pity's tear,
Now languish'd, pining for the love-sick swain.

To ev'ry tender feeling of the soul,
A kindred tone the various breeze excites;
Th' enchanted heart yields to the mild control,
And sweetly banquets on thy soft delights.

At times the notes with gentle zephyrs rise,
And trembling touch the chord of fond desire,
Now mingling, breathe in soft responsive sighs,
Then fluttering, fall, and with the gale expire.

Again the slowly-rising notes assail—
As if some tender maid, unseen, unknown,
Sigh'd for neglect—yet tuneful swell'd the gale,
To melt th' unfeeling heart with sorrow's plaintive moan.

If e'er a breast was by soft passion mov'd,
If e'er it felt Love's sympathetic fire,
With mine thy strains it cordially approv'd,
And breath d in chorus to thy praise, sweet Lyre!

A sudden gust now sweeps thy trembling strings—
What wild luxuriance undulates the air?
The swell majestic all its grandeur brings,
And dying gales their softer tribute bear.

To yonder copse why should I anxious rove,
To hear its songsters hail the new-born day?
Why pensive court the music of the grove?
Thy charming airs surpass their sweetest lay.

When vernal showers refresh the parched vale,
And Flora's train in richest hues appear,
Not more their varied tints the eye regale,
Than thy ecstatic notes delight the ear.

Should adverse winds the ruffled soul assail—
Impassioned looks the rising storm presage—
Thy soothing airs, mellifluous, cannot fail
To calm each ranc'rous passion's keenest rage.

When nature bids the busy world to close,
And silence reigns, obedient to her power,
Thy grateful murmurs, lulling to repose,
Beguile the solemn gloom of midnight hour.

Though not endowed with lively sensibilities, nor with taste for art or poetry, he was deeply moved by simple melodies, and would sit absorbed and spell-bound by certain favourite airs. In his journal for 1795, he has stated that, with another Friend, he "drew up a petition to the yearly meeting, soliciting permission to use music under certain limitations."

Before the year 1796, there exist no traces in his papers that he had devoted attention to the study of chemistry. In February of that year, he mentions the circumstance of Dr. Garnet* from Harrogate delivering a course of twelve lectures on Natural Philosophy, and thirty on Chemistry, which he was attending; and in June of the same year he writes to his brother: "I have had some thoughts of delivering a course of lectures at Kendal this summer, as far as the apparatus there would admit, with what additions might be made for the occasion. About six lectures on chemistry and six on the other branches would be my plan. Twenty subscribers at half a guinea would be a sufficient inducement to commence." March 21, 1803, he informes his brother: "I have been, as usual, fully engaged in all my leisure hours, in the pursuit of chemical and philosophical inquiries. Even my Christmas vacation was taken up in this way; indeed I have had considerable success of late in this line, having got into a *track that has not been much trod in before.*" My business continues as full as I desire, especially private teaching, having usually four lessons a day to give, besides the daily class. I have lately had the pleasure of undertaking to instruct some very amiable and accomplished young ladies in chemistry, namely, Dr. Percival's daughters."

During this year he had the honour of being invited to lecture before the Royal Institution; and after his return to Manchester, February 1, 1804, thus describes his success.

"Dear Brother,—I have the satisfaction to inform thee that I returned safe from my London journey last seventh day, having been absent six weeks. It has on many accounts

* This gentleman seems to have preceded Sir H. Davy as Professor of Chemistry at the Royal Institution.—Dr. Davy's *Life of Sir H. Davy*, p. 83.

been an interesting *vacation* to me, though a very laborious one. I went in a great measure unprepared, not knowing the nature and manner of the lectures in the Institution, nor the apparatus. My first was on Thursday, December 22, which was introductory, being entirely written, giving an account of what was intended to be done, and natural philosophy in general. All lectures were to be one hour each, or as near as might be. The number attending were from one to three hundred of both sexes, usually more than half men. I was agreeably disappointed to find so *learned* and *attentive* an audience, though many of them of rank. It required great labour on my part to get acquainted with the apparatus, and to draw up the order of experiments, and repeat them in the intervals between the lectures, though I had one pretty expert to assist me. We had the good fortune, however, never to fail in any experiment, though I was once so ill-prepared, as to beg the indulgence of the audience, as to part of the lecture, which they most handsomely and immediately granted me by a general plaudit. The scientific part of the audience was wonderfully taken with some of my original notions relative to heat, the gases, &c., some of which had not before been published. Had my hearers been generally of the description I had apprehended, the most interesting lectures I had to give, would have been the least relished,—but as it happened, the expectation formed had drawn several gentlemen of first-rate talents together; and my eighteenth, on heat and the laws of expansion, &c., was received with the greatest applause, with very few experiments. The one that followed, was on *mixed elastic fluids,* in which I had an opportunity of developing my ideas, that have already been published on the subject, more fully. The doctrine has, as I apprehended it would, excited the attention of philosophers throughout Europe. Two journals in the German language came into the Royal Institution, whilst I was there, from Saxony, both of which were about half filled with translations of the papers I have written on the subject, and comments upon them.

"Dr. Ainslie was occasionally one of my audience, and

his sons constantly; he came up at the concluding lecture, expressed his high satisfaction, and he believed it was the same sentiment with all or most of the audience. . . . I was at the Royal Society one evening, and at Sir Joseph Banks's another. This gentleman I had not, however, the pleasure of seeing, he being indisposed all the time I was in London.

"I saw my successor, William Allen, fairly launched; he gave his first lecture on Tuesday preceding my conclusion. I was an *auditor* in this case, the first time, and had an opportunity of surveying the audience. Amongst others of distinction the Bishop of Durham was present. . . . . .

"In lecturing on optics I got six ribbands, blue, pink, lilac, and red, green and brown, which matched very well, and told the curious audience so. I do not know whether they generally believed me to be serious, but one gentleman came up immediately after and told me he perfectly agreed with me; he had not remarked the difference by candle-light.

"The rain has been 27½ inches last year."

It was on this occasion that he first made acquaintance with Davy, whom he thus describes in a letter to Mr. John Rothwell:—

"London, January 10, 1804.

"I was introduced to Mr. Davy, who has rooms adjoining mine (in the Royal Institution); he is a very agreeable and intelligent young man, and we have interesting conversations in an evening; the principal failing in his character as a philosopher is that he does not smoke. Mr. Davy advised me to labour my first lecture; he told me the people here would be inclined to form their opinion from it; accordingly I resolved to *write* my first lecture wholly; to *do* nothing, but to tell them what I would do, and enlarge on the importance and utility of science. I studied and wrote for near two days, then calculated to a minute how long it would take me reading, endeavouring to make my discourse about fifty minutes. The evening before the lecture, Davy and I went into the theatre; he made me read the whole of

it, and he went into the furthest corner; then he read it and I was the audience; we criticised upon each other's method. Next day I read it to an audience of about 150 or 200 people, which was more than were expected. They gave a very general plaudit at the conclusion, and several came up to compliment me upon the excellence of the introductory. Since that I have scarcely written any thing: all has been experiment and verbal explanation. In general my experiments have uniformly succeeded, and I have never once faltered in the elucidation of them. In fact I can now enter the lecture room with as little emotion nearly as I can smoke a pipe with you on Sunday or Wednesday evening."

Dalton never became an attractive and popular lecturer. Intelligent and well-prepared hearers could not fail to be delighted with his large stores of knowledge, his original speculations, and straightforward manly sense. But he never possessed the art of devising or executing impressive illustrative experiments; and in my remembrance, as often failed as succeeded in performing those elementary experiments which he did attempt.

Shortly after, February 26, 1804, he writes:—"Since my return I have been gradually increasing in business, till it has become of such amount as to press me very hard; besides, I cannot omit the opportunity the season affords of making such experiments as require ice. My lately published essays on gases, &c., together with the more recent ones read at our Society, and of which I gave the result in my late lectures, have drawn the attention of most of the philosophers of Europe. They are busy with them at London, Edinburgh, Paris, and in various parts of Germany, some maintaining one side and some another. The truth will surely out at last."

The more recent essays, to which he here alludes, were read before the Manchester Society in the years 1802 and 1803, but were not published before the end of 1805. The first of these, an "Experimental Inquiry into the proportion of the several gases or elastic fluids constituting the atmosphere," read November 12, 1802, has peculiar interest, as

being his earliest contribution to pure chemistry, and as announcing in the combinations of oxygen and nitrous gas the first example of the law of *multiple proportions*. The specific objects of the essay were: "1. To determine the weight of each simple atmosphere abstractedly; 2. To determine the relative weights of the different gases, in a given volume of atmospheric air, such as it is at the earth's surface; and, 3. To investigate the proportions of the gases to each other, such as they ought to be found at different elevations above the earth's surface."

He discusses the various eudiometrical processes then in use, and shows that the nitrous gas method, when properly conducted, is "not only the most elegant and expeditious of all the methods hitherto used, but also as correct as any of them." 100 measures of common air, he found, would combine, in a narrow tube, with 36 of pure nitrous gas, forming nitric acid; or with 72, in a wide vessel, forming nitrous acid." The residuum in each experiment was 79 or 80 measures of pure azotic gas. These facts clearly point out the theory of the process: the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity." His results with Volta's eudiometer were not accurate, as he found that 100 parts oxygen combined with only 185 hydrogen. Probably both the gases were very remote from chemical purity. His general conclusion from his own experiments and those of Davy, was, that 100 parts of air consist of 79 azote and 21 oxygen, numbers nearly accordant with those of Dumas.

He concludes his essay with a few remarks on the proportions of oxygen and nitrogen, at various elevations above the surface of the earth. "I have little doubt as to the fact of oxygenous gas observing a diminishing ratio in ascending; for the atmospheres being independent on each other, their densities at different heights must be regulated by their specific gravities. It would be found that at the height of Mont Blanc, the ratio of oxygenous gas to azote, in a given volume of air, would be nearly as 20 to 80; consequently it follows that at any ordinary heights, the difference in the

proportions will be scarcely if at all perceptible." In a note he alludes to Gay-Lussac's having determined the constitution of air, brought from an elevation of four miles, to be the same as that at the earth's surface.

It will be most convenient to depart from the chronological sequence of his essays, and to notice here his memoir "On the constitution of the atmosphere," published in the *Philosophical Transactions for* 1826, which contains his matured views on this question. This paper consists exclusively of theoretical considerations. He recurs, and for the last time, to the question, "whether the uniform diffusion of elastic fluids through each other is occasioned by the repulsion of the elementary particles of the same kind, which appears to force them through most bodies, as well solid and liquid as aërial, except glass and the metals; or whether it is caused by attraction or chemical affinity." *Difficulties attend both views.* "I have long been inclined to adopt the former notion, as most consistent with the phenomena." He proposes an imaginary case of two equal cylindrical tubes, A and B, of indefinite length, close at the bottom and open at the top; A containing an atmosphere of hydrogen equal to 30 inches of mercury, and B an atmosphere of carbonic acid of the same pressure. The height of the hydrogen atmosphere, supposed of uniform density, would be 66 miles, that of the carbonic acid about 3·3 miles, or the heights would be "in the ratio of 20 to 1 nearly."* Afterwards, when the atmospheres were expanded to their natural extent, equal elasticities of the two gases would also be at altitudes as 20 to 1; that is, if at two miles of elevation the carbonic acid atmosphere supported 15 inches of mercury, that of hydrogen would support the same at 40 miles elevation." Imagine now lateral communications to be opened between the two tubes at various intervals, then "the two gases would intermix, and finally obtain such equilibrium that one-half of the gas at first in each division would pass into the opposite division, and the other half remain where it was.

"In the lowest division we should find equal volumes of

* According to the atomic numbers now received, in the ratio of 22 to 1 exactly.

carbonic acid and hydrogen. At the height of two miles we should find about one volume of carbonic acid mixed with two of hydrogen; at the height of four miles the carbonic acid would be to the hydrogen as one to four nearly; and at the height of forty miles, there would probably be no carbonic acid at all in either tube, but the hydrogen would then be of one-half the density it was in the lowest division." He concludes, "it appears to me as completely demonstrated as any physical principle, that whenever two or more such gases or vapours, as we have been describing, are put together, either into a limited or unlimited space, they will finally be arranged each as if it occupied the whole space, and the others were not present; the nature of the fluids and gravitation being the only efficacious agents."

Applying this doctrine to the earth's atmosphere, in a quiescent state: "1. The volumes of each gas found at the surface of the earth are proportional to the whole weights of the respective atmospheres. 2. The altitude of each atmosphere differs from that of every other, and *the proportions of each in the compound atmosphere gradually vary in the ascent.* How the case would be with regard to the earth's atmosphere, such as it actually is, in a state of continual motion and agitation, greater or less in all its parts, it is not very easy to ascertain; and it is, besides, rather a question to be decided by experiment and observation than by any theory."

A sequel to this essay was read, June 15, 1837, and published in the *Philosophical Transactions* for that year. He describes numerous experiments on air, obtained by himself from the summit of Helvellyn; by a friend, at various stations in Switzerland, about 6000 feet above the sea; and by Mr. Green, in a balloon, at elevations of 9600 feet and 15,000 feet. This last air yielded 20·59 and 20·65 oxygen per cent.; while air collected in the town the same day, and fired with the same phial of hydrogen as the preceding, gave 20·95 on the average of five experiments. He concludes, from all his experiments instituted on various occasions during a period of forty years, "that in elevated regions

the proportion of oxygen to azote is somewhat less than at the surface of the earth, but not nearly so much as the theory of mixed gases would require; and that the reason for this last must be found in the incessant agitation in the atmosphere from winds and other causes." This memoir will be read with peculiar interest; both as recording Dalton's deliberate and final opinions on a question he had considered during a large portion of his life, and as his latest production, before the paralytic seizure which permanently weakened his mental powers. In his letter book, I find, among other brief extracts from a letter to Mr. Johnston, February, 1836, the following notice on Atmospheric Air: "Will it not be thought remarkable that in 1836 the British chemists are ignorant whether *attraction, repulsion,* or *indifference* is marked when a mixture of any proportions of azote and oxygen is made."

Even in the present state of chemistry, there exists perhaps no eudiometrical process sufficiently delicate to determine this point finally and unequivocally; but all the experimental evidence we do possess is in favour of the identity of the composition of atmospheric air at all attainable elevations. Kämtz has constructed a table* showing that, on the Daltonian hypothesis, the per-centage of oxygen in the atmosphere, ascertained to be 21 at the surface of the ground, should diminish to 20·070 at a height of 10,000 Parisian feet, and to 19·140 at an elevation of 20,000 feet. But Gay-Lussac found, in air collected at a height of upwards of 21,000 feet, and in Paris, the same per-centage of oxygen; and Brunner more recently obtained 20·915 from air from the summit of the Faulhorn. There remains therefore only as grounds for suspending a decision unfavourable to the doctrine of Dalton the disturbing influence of the great polar and equatorial

* Liebig's *Handwörterbuch,* vol. i. pp. 569, 570, and Kämtz, *Lehrbuch,* B. i. p. 48. Kämtz, however, admits the disturbing influence of aërial currents; and declines pronouncing against the hypothesis of Dalton, upon the insulated analysis of Gay-Lussac. He regards numerous fresh eudiometrical experiments, at heights differing some thousand feet, as necessary to the final adjudication of the question.—p. 49.

currents.* Other theoretical objections have been already urged to his first unmodified hypothesis.† Gmelin regards this, as well as the rival theories of Berthollet and Thomson, as "likely to be soon forgotten." He refers the phenomena of gaseous mixtures to "adhesion, or that kind of attraction, which acts at infinitely small distances, and between bodies of different natures, giving rise to the union of these bodies into a heterogeneous whole, called a mixture or mechanical combination."‡

We now return to the next in the series of his early memoirs, "On the tendency of elastic fluids to diffusion through each other," read Jan. 28, 1803. These experiments were suggested by his theory of the constitution of mixed elastic fluids. Dr. Priestley had suggested that, though gases when mingled, speedily diffuse themselves equably through one another without regard to their specific gravities, it might be possible to bring them into contact with so little agitation as that they would remain separate. Dalton shows, by a few simple experiments, that this is impossible. Two phials, filled with different gases, were connected together by a glass tube 10 inches long and $\frac{1}{20}$ inch bore. In all cases, the heavier gas was in the under phial; yet, after the lapse of a certain time, the gases were uniformly diffused through each other in both phials. It cannot now be expedient to detail experiments merely destined to establish the broad fact of mutual diffusion; for Professor Graham has since investigated the phenomena of diffusion with extreme precision, and has determined the beautiful law that the *diffusive volumes* are inversely as the square root of the densities of the gases. It is remarkable that this law is identical with that of the velocities of gases rushing into a vacuum. Hence it has been regarded, but, as Professor Graham has shown, incorrectly,§ as furnishing confirmation of Dalton's theory of mixed gases.

* Graham's *Elements of Chemistry*, vol. i. p. 333.
† *Supra*, page 33.
‡ Gmelin's *Handbook*, vol. i. pp. 20, 21.
§ Graham's *Elements*, vol. i. pp. 88, 89.

In his memoir "On the absorption of gases by water and other liquids," read October 21, 1803, he again departs from the analytical mode of exposition, and affirms, *in limine*, that "if a quantity of water, freed from air, be agitated in any kind of gas, not chemically uniting with water, it will absorb its bulk of the gas, or otherwise a part of it equal to some one of the following fractions—namely, $\frac{1}{8}$, $\frac{1}{27}$, $\frac{1}{64}$, $\frac{1}{125}$, &c., these being the cubes of the reciprocals of the natural numbers 1, 2, 3, &c., or $\frac{1}{1^3}$, $\frac{1}{2^3}$, $\frac{1}{3^3}$, $\frac{1}{4^3}$, &c., the same gas always being absorbed in the same proportion, as exhibited in the following table:—

	Dalton.*	Saussure.†
Carbonic acid	1·00	1·06
Sulphuretted hydrogen	1·00	2·53
Nitrous oxide	1·00	0·76
Olefiant gas	·125	·155
Oxygen	·037	·065
Nitrous gas	·037	·042
Carbonic oxide?	·037	·062
Hydrogen	·0156	·046

"It must be understood that the quantity of gas is to be measured *at the pressure and temperature with which the impregnation is effected.*" "There are two very important facts contained in this, the second article. The first is that the quantity of gas absorbed is as the density or pressure. This was discovered by Mr. William Henry, before either he or I had formed any theory on the subject. The other is, that the density of the gas in the water has a special relation to that out of the water, the distance of the particles within being always some multiple of that without. Thus, in the case of carbonic acid, &c., the distance within and without is the same, or the gas within the water is of the same density as without; in olefiant gas the distance of the particles in the water is twice that without; in oxygenous gas, &c., the distance is just three times as great within as without; and in azotic, &c., it is four times. This fact was

* Dalton, *New System*, vol. i. p. 201.

† Poggendorff, Art. "Absorption," pp. 29, 30, 46, *Handwörterbuch der Chemie*, vol. i.

the result of my own inquiry. The former of them, I think, decides the effect to be mechanical; and the latter seems to point to the principle on which the equilibrium is adjusted."

His theoretical conclusions from his own experiments and from the previous ones of my father—(published *Phil. Trans.* for 1803)—were that, 1. "All gases that enter into water and other liquids by means of pressure, and are wholly disengaged again by the removal of that pressure, are *mechanically* mixed with the liquid, and not *chemically* combined with it. 2. Gases so mixed with water, &c., retain their elasticity or repulsive power amongst their own particles, just the same in the water as out of it, the intervening water having no other influence in this respect than a mere vacuum. 3. Each gas is retained in water by the pressure of gas of its own kind incumbent on its surface; abstractedly considered, no other gas with which it may be mixed having any permanent influence in this respect . . . 7. An equilibrium between the outer and inner atmospheres can be established in no other circumstance than that of the distance of the particles of one atmosphere being the same, or some multiple of that of the other; and it is probable the multiple cannot be more than 4; for in this case the distance of the inner and outer atmospheres is such as to make the perpendicular force of each particle of the former on those particles of the latter that are immediately subject to its influence, physically speaking, equal." In his *New System* (vol. i., p. 197) Dalton returned to this subject, and still maintained the existence of the numerical relation, already stated, between the gas absorbed and that incumbent on the liquid; but the subsequent experiments of Saussure are totally irreconcilable with this hypothesis, as may be gathered from simple inspection of the annexed table,* in which the observed and calculated volumes of each gas, absorbed by one volume of water, are compared. Indeed, Dalton himself admitted that, with respect to the less absorbable gases, theory and experience do not perfectly coincide. Thus water, he afterwards found, may be impregnated by brisk agitation with $\frac{1}{50}$ instead of $\frac{1}{64}$

* Page 56.

of hydrogen and azote. These anomalies he ascribed to the viscidity of the aqueous particles, which detains a number of small bubbles of the gas, independently of those which are confined by exterior pressure.

The simple relation, affirmed by Dalton to subsist between the particles of a gas, absorbed by water and other liquids, and the superincumbent particles of the same gas, must therefore be abandoned as inconsistent with experimental evidence. Even his general doctrine, that in all those cases in which the absorbed gas retains the distinguishing properties of an elastic fluid, remaining in the liquid only so long as it is confined by pressure of gas of the same kind, the absorption is due to mechanical and not to chemical forces; if still tenable, is not so absolutely and in its primitive form. Thus, "since Saussure's experiments have demonstrated that the relation in which any two gases are separately absorbed by a liquid varies with the nature of that liquid, whereas the relation ought to be constant, if the amount of absorption of a gas, considered in reference to various liquids, depended merely upon the size of the pores of these liquids; Dalton's notion that 'the water appears passive in the business' cannot be rigidly maintained." Poggendorff concludes his elaborate article on absorption of gases, both by solids and liquids, with the judgment, that the elements are wanting, in the present state of science, for the absolute and final determination of the true theory.*

The concluding paragraph of the memoir on Absorption, as being the first announcement of his greatest discovery, must be here introduced in his own words.

"8. The greatest difficulty attending the mechanical hypothesis, arises from different gases observing different laws. Why does water not admit its bulk of every kind of gas alike? This question I have duly considered, and though I am not yet able to satisfy myself completely, I am nearly persuaded that the circumstance depends upon the weight and number of the ultimate particles of the several gases,

* "Absorption."—Liebig's *Handwörterbuch*, B. i. pp. 62, 63.

those whose particles are lightest and single being least absorbable, and the others more, accordingly as they increase in weight and complexity.* An inquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new. I have lately been prosecuting this inquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results, as far as they appear to be ascertained by my experiments."

TABLE (*read Oct.* 1803, *but published Nov.* 1805)

OF THE RELATIVE WEIGHTS OF THE ULTIMATE PARTICLES OF GASEOUS AND OTHER BODIES.

Hydrogen	1	Nitrous oxide	13·7
Azote	4·2	Sulphur	14·4
Carbon	4·3	Nitric acid	15·2
Ammonia	5·2	Sulphuretted hydrogen	15·4
Oxygen	5·5	Carbonic acid	15·3
Water	6·5	Alcohol	15·1
Phosphorus	7·2	Sulphurous acid	19·9
Phosphuretted hydrogen	8·2	Sulphuric acid	25·4
Nitrous gas	9·3	Carburetted hydrogen	6·3
Ether	9·6	Olefiant gas	5·3
Gaseous oxide of carbon	9·8		

This memoir, with its significant conclusion, worthily terminates the first decennial period of Dalton's life in Manchester; a period in which his genius was pre-eminently fertile of great ideas. It seems a fitting moment to pause in our narrative, in order to survey what he had already achieved; to trace the strictly logical filiation of thought in his successive discoveries; and especially to define the *genesis* of his capital and master-idea,—the grand atomic generalization.

It is obvious, that his birth and early residence among the lakes and mountains of Cumberland, a region peculiarly exposed to sudden and impressive atmospheric changes, gave the first special impulse to his genius. His earliest attempts in science were general remarks on the weather (1787). From this starting-point he advanced, in about six months,

* "Subsequent experience renders this conjecture less probable."

after constructing, with his own hands, the three necessary instruments, to the systematic study of the *atmosphere* in its three main variable conditions of weight, temperature, and moisture. During the twelve following years, he was satisfied with patiently observing and methodically recording these atmospheric states. The last of the three, viz., the relation of air to moisture, was the first to attract his scrutiny, as a question of high theoretical import. It now becomes essential to the just appreciation of what he accomplished, to glance rapidly at the then existing state of meteorological science, and especially of hygrometry. Of this, I apprehend that the classical work of the illustrious Saussure, *Sur l'Hygrometrie*, published in 1783, must be regarded as the best exponent. Saussure describes three different processes which had been employed, at that period, to ascertain the absolute quantity of humidity present in the air. The first consisted in exposing to the action of moist air, bodies capable of absorbing its humidity, and estimating its amount by the changes of weight, of volume, or of figure, which it induced in those bodies. The second was to immerse in air, water, or bodies moistened with it, and to determine the quantity evaporated. The third was to condense by cold, the vapours suspended in the atmosphere, and to estimate its humidity either by the actual quantity of vapour which was condensed, or by the degree of cold required to effect this change. This last, in the simple form of gradually cooling water of atmospheric temperature by snow or ice, till a slight dew *begins* to be formed on the sides of the containing vessel, was the method of M. le Roi, and was afterwards adopted by Dalton.

It is well known that the hygrometer used by Saussure, consisted of a hair, moving, by its contractions and dilatations, an index affixed to a graduated dial plate. The point of extreme humidity is obtained by inclosing the instrument for about an hour in a receiver, whose sides are kept constantly moistened with water; that of extreme dryness by surrounding a receiver, inclosing the hygrometer, with a cylinder of iron, which was coated with a salt, formed by

exposing nitre and crude tartar to a red heat. It is manifest, that an instrument constructed on this principle, can only furnish *relative* indications of the comparative dryness of air at various temperatures; and that Dalton was justified in affirming, in 1793, that "to ascertain the exact quantity of water in a given quantity of air, is, I presume, an object not yet fully attained.*"

Even in his boyish days, we have marked Dalton impelled by a taste, resembling instinct in its strength and directness, towards exact numerical determinations. Such a mind could never rest satisfied with merely *relative* admeasurement, or with any result short of the *absolute* weight of vapour present in a given volume of air, at every attainable temperature. This great problem had doubtless been often, as his habit was, the object of calm, steadily persistent meditation, between the year 1787, when he constructed his first simple hygroscope of whipcord, and the year 1793, when, in treating of the hygrometer, he wrote the passage just quoted. Yet year after year, and day by day, we track him in his journal, patiently noting the weight, temperature, and moisture of the atmosphere, the direction of the winds, and the amount of rain, and sternly abstaining from all hasty generalization. It was not till he had been twelve years† an observer, in March, 1799, that he distinctly announced the true theory of aqueous vapour,‡ and rose from the rank of a servant to that of an interpreter of nature. This happy thought was experimentally worked out in his essays on the *Force of Vapour and Evaporation*, 1801; and the momentous problem of the absolute relations of air and moisture was exactly and finally resolved.

The relation of air and aqueous vapour constitutes obviously only a single special example of the universal relations

* *Meteorological Observations*, page 30.

† It is true that six years previously, in his *Meteorological Essays*, p. 131, 1793, he alludes to "the opinion, which to me *appears the more probable*, that water evaporated is not chemically combined with the aërial fluids, but exists as a peculiar fluid diffused among the rest." But in the course of the Essay, he speaks of this question as a matter "not clearly ascertained."

‡ *Manchester Memoirs*, vol. v. p. 351, note, already quoted.

of mixed elastic fluids, and there is every reason to believe that Dalton's mind ascended immediately from the special case to the general doctrine. We have seen, that in their exposition he has given precedence to the latter, devoting Essay I. to the constitution of mixed gases, and Essay II. to the force of steam. From this grand conception of the nature of mixed elastic fluids, we trace an obvious and natural filiation of thought through his eudiometrical memoir in 1802, his inquiry into the tendency of elastic fluids to mutual diffusion, in 1803, to the last of his researches (on the absorption of gases by water, also in 1803), which is included in the same decennial period.

To the same parentage we may now trace his first vision of the atomic constitution of matter. It is impossible to peruse the essay on the constitution of mixed gases, and especially to contemplate the plate, by which it is illustrated, without perceiving that meditation on the constitution of homogeneous and mixed elastic fluids had impressed his mind with a distinct picture of self-repellent particles or atoms. Thus, he affirms, "homogeneous elastic fluids are constituted of *particles,* that repel one another with a force decreasing directly as the distance of their centres from each other." Again :—"It follows, too, that the distances of the centres of the *particles,* or, which is the same thing, the diameters of the spheres of influence of each *particle,* are inversely as the cube root of the density of the fluid."* But the plate, which is now reproduced,† furnishes ocular demonstration that it was, in contemplating the essential condition of elastic fluidity, that he first distinctly pictured to himself the existence of atoms. As, however, the origin of this great conception is doubtless the most interesting circumstance in his life, I copy verbatim the following minute in my father's handwriting, dated 1830, February 13, of a conversation with Mr. Dalton :—"Mr. Dalton has been settled in Manchester thirty-six years. His volume on meteorology, printed but not published before he came here. At p. 132 *et seq.*

* *Manchester Memoirs,* vol. v. pp. 540, 602.

† See plate at the end of Appendix.

of that volume, gives distinct anticipations of his views of the separate existence of aqueous vapour from atmospheric air. At that time the theory of chemical solution was almost universally received. *These views were the first germs of his atomic theory, because he was necessarily led to consider the gases as constituted of independent atoms.* Confirmed the account he before gave me of the origin of his speculations, leading to the doctrine of simple multiples, and of the influence of Richter's table in exciting these views." Thus far, then, we can trace a natural filiation of thought, in unbroken sequence, from—1. The vigilant and persistent observation of meteorological phenomena, and specially of the variations *of the atmosphere* in weight, temperature, and *moisture*; to 2. The theory of the relations of air and vapour and of mixed gases; and finally, to the abstract conception of elastic fluidity, and of self-repulsive molecules or atoms. There remained, however, a wide space to be traversed, from this general physical conception of the existence of atoms, to the experimental establishment of *the relative weights* of the ultimate particles of various chemical elements and compounds, announced by him two years afterwards,—October 1803. But it will be convenient to delay the further consideration of the atomic doctrine till we reach that chapter in the new system of chemical philosophy in which it is clearly but most succinctly expounded.

In February, 1805, he made a short stay in London to purchase apparatus for his lectures. On his way he visited Birmingham, and dined with James Watt, "that veteran in science, with whom I spent some hours most agreeably." In the summer of this year he delivered a course of lectures in Manchester, and had an audience of about 120, at two guineas each. Towards the close of the year he went to reside in George-street, at the house of the Reverend Wm. Johns, with whom he continued to lodge and board for the greater part of his life. Miss Johns has thus recorded the characteristic simplicity with which this engagement was formed:—"As my mother was standing at her parlour-window one evening towards dusk, she saw Mr. Dalton pass-

ing on the other side of the street, and on her opening the window, he crossed over and greeted her. 'Mr. Dalton,' said she, 'how is it that you so seldom come to see us?' 'Why, I don't know,' he replied, 'but I have a mind to come and live with you.' My mother thought at first that he was in jest, but finding that he really meant what he said, she asked him to call again the next day, after she should have consulted my father. Accordingly he came and took possession of the only bedroom at liberty, which he continued to occupy for nearly thirty years. And here I may mention to the honour of both, that throughout that long connection he and my father never on any occasion exchanged one angry word, and never ceased to feel for each other those sentiments of friendly interest which, on the decline into years of both, ripened into still warmer feelings of respect and affection."

He writes to his brother, November 10, 1805: "My present employment in teaching is principally in private families, many of which are half a mile or more into the country, which affords a pleasant walk very conducive to health. I contemplate a repetition of my lectures during the winter, and am preparing a work of my own for the press." This was the first part of his *New System of Chemical Philosophy,* which was, however, not published before May, 1808. In March of that year he writes to his brother:—"I have nearly finished for publication, Part I of my *New System of Chemical Elements,* which has cost me abundance of time and labour, though it is compressed into little compass." Again: —"It has cost me much time and pains, but I hope it will be found not altogether in vain." After its publication, he writes, December 11, 1809:—"I have got nearly 200 pages more of my Chemistry (Part II.) done. The former part has got into France, and been reviewed by the chemists of Paris. About two months ago I received a very handsome present from Berthollet, sent me in return for mine sent him. It was *Mém. de la Société d'Arcueil,* being the most recent transactions of the Parisian chemists. It contains some very valuable papers; they speak very respectfully of my first part."

He states in the Preface: "In 1803, the Author was gradually led to those primary laws which seem to obtain in regard to heat and to chemical combinations; and which it is the object of the present work to exhibit and elucidate. A brief outline of them was first publicly given the ensuing winter, in a course of lectures on Natural Philosophy, at the Royal Institution in London. . . . . In the spring of 1807 he was induced to offer the exposition of the principles herein contained, in a course of lectures, which were twice read in Edinburgh and once in Glasgow. On these occasions he was honoured with the attention of gentlemen universally acknowledged to be of the first respectability for their scientific attainments."

It can in no measure derogate from Dalton's just intellectual fame to admit that, in a work on chemical philosophy, written shortly after the commencement of this century, there is much both of detail, and even of general laws, which has been superseded by subsequent discovery. It could not indeed be otherwise; for in the eloquent words of Humboldt, in his noble preface to *Cosmos*:—"It has been often lamented, that while purely literary mental products have their roots in the depths of feeling and of a creative imagination, all that is connected with empiricism, with the investigation of natural phenomena and physical laws, assumes in a few decades, from the increasing precision of instruments, and the gradual extension of the horizon of observation, a new form; that antiquated scientific writings are consigned to oblivion as absolutely unreadable." Still we may indulge the conviction that, in the *New System*, as Humboldt ventures to hope for his *Cosmos*, the element of permanence (*das Beharrliche*) will triumph, and that Dalton's work is one which future ages will not willingly let die. It can no longer serve as a manual for the student; but it will never cease to be consulted and reverenced by the masters of science, as abounding in profound and original views on the higher and more abstract philosophy of heat, and as the manifest product of masculine and patient thought.

The first part of the *New System* is divided into three

chapters, each again subdivided into numerous sections. Chapter I on heat; Chapter II on the constitution of bodies, and Chapter III on chemical synthesis. It is worthy of remark, that the first section of the chapter on heat is devoted to "temperature and the instruments for measuring it." Thus we are again impelled to look back upon his early observations on atmospheric *temperatures*, as the parent stem from which this new offshoot derives its principle of life. With respect to temperature, he arrived at what he then regarded as "four most remarkable analogies:"—

1. "All pure homogeneous liquids, as water and mercury, expand from the point of their congelation, or greatest density, a quantity always as the square of the temperature from that point.

2. "The force of steam from pure liquids, as water, ether, &c. constitutes a geometrical progression to increments of temperature in arithmetical progression.

3. "The expansion of permanent elastic fluids is in geometrical progression to equal increments of temperature.

4. "The refrigeration of bodies is in geometrical progression in equal increments of time."

On these principles he constructed a new scale, or table, of temperature. (*New System*, p. 14). But MM. Dulong and Petit have shown, in the first part of their great memoir devoted to the measure of temperature, that not one of these four laws was consistent with their experiments.* Dalton himself, though emphatically self-reliant and tenacious of opinions once embraced, was not insensible to the cogency of these objections. I find from notes, taken in

* "Toutes ces lois se vérifient assez exactement dans le thermomètre de M. Dalton, pour les températures voisines de celles où la nouvelle échelle coincide avec l'ancienne; et si le même accord s'observait à toutes les températures, l'ensemble de ces lois formerait assurément l'une des plus importantes acquisitions de la physique moderne. Malheureusement l'accord dont il s'agit est fort éloigné de se maintenir dans les températures très-basses ou très-élevées, ainsi que nous allons le voir."—*Ann. de Chimie et de Phys.* t. vii. pp. 151, 152. 1817.

1823, when, as his pupil, I carefully perused the *New System* in his presence, and with his oral comments, that, though he had not then absolutely abandoned these four laws as untenable, yet that he no longer retained confidence in their truth, and was willing tacitly to yield to the weight of experimental evidence.

In the Appendix to Vol. ii. of his *New System*, published in 1827, he has given an abstract of this Memoir (p. 271, &c.), and notices their "animadversions on the general laws relating to the phenomena of heat announced in my Elements of Chemical Philosophy, together with a table, drawn up to show the discordance between the air thermometer and the mercurial thermometer, both being graduated in the manner I proposed in the said Elements." With the candour of a true philosopher, he expresses his belief that their results are good approximations to the truth, and adds: "I have made some experiments on the expansions of air above 212°, which lead me to adopt the results of Dulong." Again, "the great deviation of the scales between the temperatures of freezing water and freezing mercury is sufficient to show, as Dulong and Petit have observed, that their coincidence is only partial." (p. 289).

The second section on expansion may also be now passed over, as corrected in its details by the same exact experimenters. But the fourth section, on the theory of the specific heats of elastic fluids, announces the remarkable proposition,* that "the quantity of heat belonging to the ultimate particles of all elastic fluids, must be the same under the same pressure and temperature, and concludes with the hope, "that some principle analogous to the one now adopted, may soon be extended to solid and liquid bodies in general." This prophetic anticipation has been since, in large measure, realized. Petit and Dulong, in their *Memoir on the Theory of Heat*, 1819, showed that in twelve of the metals and in sulphur, the products of the ascertained specific heats, multiplied by the atomic weights, did not differ from each other more than might be attributed to errors of observation.

* *New System*, pp. 70, 75.

These products obviously represent the atomic specific heats,* which, as Dalton conjectured, are the same, at least in those bodies. "The capacities of the three gaseous elements, oxygen, hydrogen, and nitrogen, may likewise be adduced in support of such a relation, provided they are the same for equal volumes of the gases, agreeably to the observations of Dulong."—*Graham*, p. 137. Neumann has since discovered a similar relation in certain classes of compounds. Thus the specific heats of the atoms of several carbonates were nearly identical; as of several sulphates; but the number denoting the specific heat of the atoms of carbonates did not accord with that of the sulphates. The researches of M. Regnault also indicate the existence of such relation in several analogous compounds.† There are, it is true, various exceptions which cannot, in the present state of our knowledge, be reconciled with the law. There seems no reason to doubt, that MM. Petit and Dulong had derived this important idea from the *New System*, which they had obviously studied with great care, and many doctrines of which they successfully controvert.

Dalton again recurs to this question in the Appendix to his second volume, and with singular inconsistency thus refuses his adhesion to the doctrine of equality of heat, in the atoms of matter in the solid and liquid state. "If M. Dulong would assume all his simple elements in an elastic state, and under one uniform pressure, the hypothesis would then make a part of mine (Vol. i. p. 70), and there is great reason to believe it would be either accurately true, or a good approximation; but to suppose that some of the bodies should be in a solid state, having their particles united by various degrees of attraction, others fluid, and others in the elastic state, without any material modifications of their heat arising from these circumstances, appears to me to be in opposition to some of the best established phenomena in the mechanical philosophy," (p. 295).

* Cor. 1. The specific heats of equal *weights* of any two elastic fluids are inversely as the weights of their atoms or molecules.—*New System*, p. 72.

† Graham's *Elements*, p. 139; *Gmelin*, p. 243, n.

Section 8, on the temperature of the atmosphere, is an ingenious attempt to explain the fact "that the atmosphere in all places and seasons is found to decrease in temperature in proportion as we ascend, and nearly in an arithmetical progression." Mr. Dalton suggests, as most probable, that "the natural equilibrium of heat in an atmosphere, is when each atom of air in the same perpendicular column is possessed of the same quantity of heat; and consequently the natural equilibrium of heat in an atmosphere is when the temperature gradually diminishes in ascending. That this is a just consequence cannot be denied, when we consider that air increases in its capacity for heat by rarefaction, when the quantity of heat is given or limited, therefore the temperature must be regulated by the density." It is not difficult to trace the parentage of this idea to his theory of the specific heat of elastic fluids, which, we have just seen, affirms the quantity of heat belonging to the ultimate particles of all elastic fluids to be the same under the same pressure and temperature. Dr. James Hutton,* whose ingenious theory of rain, so far in advance of the science of his day, was signally confirmed by Dalton's researches on the force of vapour, had evidently arrived at the same explanation as the above, of the decrease of temperature, in ascending from the surface of the earth. "It is well known that the condensation of air converts part of the latent into sensible heat, and that the rarefaction of air converts part of the sensible into latent heat. If, therefore, we suppose a given quantity of air to be suddenly transported from the surface to any height above it, the air will expand on account of the diminution of pressure, and a part of its heat becoming latent, it will become colder than before. Thus also when a quantity of heat ascends, by any means whatever, from one stratum of air to a superior stratum, a part of it becomes latent, so that an equilibrium of heat can never be established among the strata; but those, which are less, must always remain colder than those that are more compressed." Dalton employs (p. 127)

* Playfair's *Works*, vol. iv. p. 72, note.

precisely the same mode of reasoning, but with the advantage of more exact data.

Chapter II, on the constitution of bodies, and especially Sections 1 and 2, on pure and mixed elastic fluids, contain the development of Dalton's conception of physical atoms, and constitute significant prolegomena to the important chapter on chemical synthesis. They may still be studied with advantage, and will be found to minister strong confirmation to the view that has been already taken, of the genesis, in his mind, of the atomic theory. Thus all bodies, he teaches, "are constituted of a vast number of extremely small particles or atoms of matter, bound together by a force of attraction. . . . . Besides this, we find a force of repulsion. This is now generally, and I think properly ascribed to the agency of heat. An atmosphere of this subtile fluid constantly surrounds the atoms of all bodies, and prevents them from being drawn into actual contact." A vessel full of any pure elastic fluid presents to the imagination a picture like one full of small shot. The particles of a pure elastic fluid "are constituted of an exceedingly small central atom of solid matter, which is surrounded by an atmosphere of heat, of great density next the atom, but gradually growing rarer according to some power of the distance." Again, "in prosecuting my inquiries into the nature of elastic fluids, I soon perceived it was necessary, if possible, to ascertain whether the atoms or ultimate particles of the different gases are of the same size or volume in like circumstances of temperature and pressure." He concluded, erroneously according to modern doctrine, "that every species of pure elastic fluid has its particles globular, and all of a size; but *that no two species agree in the size of their particles.*" In these and in numerous other passages of equal significance, we perceive that Dalton had attained a distinct image of physical atoms, through patient contemplation of the condition of elastic fluidity. We may now, therefore, proceed to trace the steps by which he advanced to measure their relative weights and diameters, and became the founder of a 'New System of Chemical Philosophy.'

CHAPTER IV.

ATOMIC THEORY—VIEWS OF WENZEL, RICHTER, PROUST, AND BERTHOLLET—ANTICIPATIONS OF DR. BRYAN HIGGINS AND MR. WM. HIGGINS—THEIR WORKS NOT KNOWN TO DALTON—LAW OF MULTIPLE PROPORTIONS DISCOVERED BY DALTON—STEPS LEADING TO THE ATOMIC THEORY AND DETERMINATION OF ATOMIC WEIGHTS—COMPOUND PROPORTIONS—CHAPTER ON SYNTHESIS—NUMBER OF ATOMS IN COMPOUNDS—DR. THOMSON AND DR. WOLLASTON EARLIEST CONVERTS—SIR H. DAVY'S OBJECTIONS—GAY-LUSSAC'S LAW OF VOLUMES—DR. KOPP'S EXPLANATION OF DALTON'S REJECTION OF THE LAW—RESEARCHES OF BERZELIUS—LETTER FROM BERZELIUS TO DALTON.

The ultimate constitution of matter seems to have formed an object of speculative inquiry, many ages anterior to the birth of physical science, and to have entered as an important element into most systems of ancient philosophy. Space is demonstrably infinitely divisible, and matter is conceivably so, as occupying space. But it was taught in the earliest schools of Greek philosophy, and is still regarded as probable, that there exists a practical limit to divisibility; that matter is not divisible, by the known forces of nature, further than into particles or atoms of extreme minuteness, but of definite magnitude. Mr. Colebrooke has even detected this conception in the Nyaya,—a system of Hindoo logic,—as entertained by Canadi. Indeed the profound remark of Mr. Mill,* that the agitation of the dark and subtle questions of abstract metaphysics is by no means to be regarded as a symbol of high general culture, is strictly applicable to speculations respecting the essential constitution of matter. Such doctrines stand almost at the threshold of human knowledge; they are not the slowly ripened fruits of experiment and induction,

* *History of British India*, vol. ii. p. 79.

but are struck out by inventive and imaginative minds, as readily in the infancy as in the maturity of physical science.

It is not my purpose, and would be most unadvised, in presence of the comprehensive monograph of my friend, Professor Daubeny, to trace the ancient history of the corpuscular or atomic doctrine; its admission by Newton, in the often quoted 31st query, subjoined to his treatise on optics, or its modification in the dynamic hypothesis of Boscovich and Hutton.* The atomic doctrine will be here contemplated, not as a question of general physics, but exclusively as applied by Dalton to the interpretation of chemical phenomena.

A brief notice of the facts and doctrines, respecting proportions, which were prevalent towards the close of the eighteenth century, is essential to the just appreciation of Dalton's discoveries. It is obvious that the recognition of constant combining proportions is involved in the very conception of a chemical analysis, which must be fruitless, unless the same body consist of the same constituents in the same invariable proportions. This notion, therefore, even if not expressly avowed, must have silently prompted the analytical researches of Van Helmont, Boerhaave, and the earliest chemists. The salts, and especially the large class of neutral salts, were the first objects of quantitative investigation. It had been first observed by Bergman, that two neutral salts, after mutual decomposition, give birth to products which are equally neutral; but it was reserved for Wenzel to discover the theory of this important phenomenon. Wenzel ascertained, by a numerous series of analyses, far surpassing in accuracy those of any other chemist of his time,† that the different weights of the alkalies or earths, which neutralize the same weights of any given acid, also require for their neutralization an equal quantity of every other acid; in other words, that the relative proportions between certain quantities of alkalies or earths, which saturate a given weight of one and the same acid, remain the same with all other

* Playfair's *Life of Dr. Hutton. Works*, vol. iv. pp. 84-88.

† Berzelius, B. v. p. 16 (ed. 1835).

acids:* hence the persistence of the state of neutrality after double decomposition, whether the two salts are mingled in the exact proportions necessary to entire decomposition or not. Wenzel had the rare merit of discovering all the consequences flowing from this prolific truth; he perceived that the composition of neutral salts being thus subordinate to definite laws, it is possible by the careful analysis of a few, to ascertain the constitution of many others by a simple calculation. He did not, however, pursue this important line of research, his main object having been to explain the persistence of neutrality after mutual decomposition. Richter (1789-1802) repeated the experiments of Wenzel, and extended them to a larger series of saline bodies. He was the first to arrange in a tabular form, the relative weights in which the acids and bases combine with one another. Thus the proportions by weight of all alkaline or earthy bases, which neutralize a given weight of one and the same acid, as 1000 parts of sulphuric acid, constituted one table of the saturating power of bases. A second similar table was constructed of the proportional weights of the different acids required to saturate the same weight of a given base. From these data, he showed that the constitution of all neutral salts, formed by the union of those acids and bases, can be determined. Fischer, in 1802, pointed out that Richter's tables might be reduced to a single one, and actually calculated such a table from Richter's experiments. In this, the earliest table of equivalent weights, sulphuric acid is made the standard of reference and designated 1000. Richter himself adopted the same system of arrangement, and published, in 1803, a more complete table.†

Yet these important labours of Wenzel and Richter were little esteemed at that period, even in Germany. We learn from the testimony of Berzelius and Kopp, that "it was not till long after the time when these researches were made that their merit was recognized; they remained without influence on the general condition of chemistry, until, when extended

* Kopp, *Geschichte der Chemie*, B. ii. p. 357.

† Kopp, B. ii. pp. 358—366.

and placed in a brighter light by Dalton's discoveries, they exercised the most comprehensive influence on all numerical data in chemistry." This neglect is ascribed to their being made public at a period "when the antiphlogistic theory was the subject of much controversy, especially in Germany, and when chemists bestowed little attention on all other theoretical questions." In France the genius and eloquence of Berthollet were exerted in support of the diametrically opposite doctrine; that the elements have their maximum and minimum, beyond which combination is impossible; but that within those limits they can combine in all proportions. Berthollet's views were, however, strenuously opposed by his countryman Proust, who, after a controversy extending through several years (1801-8), is admitted to have unequivocally demonstrated their unsoundness. By a series of skilful and very accurate analyses of oxides, sulphurets, and salts, Proust established the fact, that the metals do not combine with oxygen or sulphur in more than two or three fixed proportions, and that the absorption of intermediate portions of oxygen is due to the conversion of successive quantities of the lower oxide into the state of higher oxide, which is merely mechanically mixed with the lower oxide. We may, therefore, according to Kopp, regard the year 1808 as the period when the law of definite or invariable proportions was irrevocably established by Proust and his German predecessors. It is also obvious that what is now called the law of reciprocal proportion (viz., that if the combining weights of any series of bodies, A, B, C, &c., are known, and the proportions in which A combines with R, S, T, &c., are ascertained, then if B, C, D, &c., are capable of entering into *analogous* combinations with R, S, T, &c., the combining weights will be identical with those of their combinations with A) is involved in Wenzel's interpretation of persistent neutrality and in the construction of Richter's table of equivalents. But it would be a serious error to suppose that these great laws were then recognized as abstract and universal truths, in the distinct and bright light in which they stand manifested to us by the theoretical interpretation of Dalton. At the close of the

last century, the laws of definite and reciprocal proportions were, at most, dimly and faintly perceived by a few advanced but isolated inquirers; and, as unexplained empirical laws, had no impressiveness or significance for the pure reason.

Most chemical writers have ascribed to Mr. William Higgins the merit of first applying the corpuscular or atomic hypothesis to the interpretation of these empirical laws. But Sir H. Davy, in his *Discourse* for 1826, has shown that Dr. Bryan Higgins, the relative and instructor of that gentleman, had maintained, so early as 1786, "that elastic fluids unite with each other in limited proportions only; and that this depends upon the combination of their particles or atoms with the matter of fire, which surrounds them as an atmosphere, and makes them repulsive of each other; and he distinguishes between simple elastic fluids, as composed of particles of the same kind, and compound elastic fluids, as consisting of two or more particles combined, in what he calls molecules, definite in quantity themselves, and surrounded by definite proportions of heat." Mr. William Higgins, indeed, states in the preface to his work, entitled *A Comparative View of the Phlogistic and Antiphlogistic Theories with Inductions*, "I am indebted to Dr. Higgins, who is a phlogistian, for my first instructions in chemistry," (p. xii. 2nd edition, 1791). I had carefully perused, many years ago, this volume, and had extracted the following passages, in which I have translated the obsolete terms used by Mr. Higgins, into the language of modern chemistry, as the most striking anticipations it contains, of atomic views. Mr. Higgins thus expresses himself (pp. 36 and 37) respecting the combinations of sulphur and oxygen: "100 grains of sulphur, making an allowance for water, require 100 or 102 of the real gravitating matter of oxygen to form sulphurous acid gas, and as this gas is little short of double the specific gravity of oxygen, we may conclude that the ultimate particles of sulphur and oxygen contain equal quantities of solid matter, for oxygen suffers no considerable contraction by

uniting to sulphur in the proportion merely necessary for the formation of sulphurous acid. Hence we may conclude that in sulphurous acid a single ultimate particle of sulphur is intimately united only to a single particle of oxygen, and that, in sulphuric acid, every single particle of sulphur is united with two of oxygen, being the quantity necessary to saturation." Still more in conformity with modern doctrine is his view of the composition of water. "As two cubic inches of hydrogen require but one of oxygen to condense them, we must suppose that they contain an equal number of divisions [atoms], and that the difference of their specific gravity depends chiefly on the size of their ultimate particles, or we must suppose that the ultimate particles of hydrogen require two or three or more of oxygen to saturate them. If this latter were the case, we might produce water in an intermediate state, as well as sulphuric or nitrous acids, which appears to be impossible; for in whatever proportion we mix our airs, or under whatsoever circumstances we combine them, the result is invariably the same. This likewise may be observed with respect to the decomposition of water. Hence we may justly conclude that water is composed of molecules, formed by the union of a single particle of oxygen to an ultimate particle of hydrogen, and that they are incapable of uniting to a third particle of either of their constituent principles" (pp. 37 and 38). Equally meritorious was his sagacious anticipation of the composition of the nitrous compounds. "I am of opinion that in nitrous gas, every primary particle of azote is united to two of oxygen, and that these molecules are surrounded by one common atmosphere of fire." He has given a diagram exhibiting the mode in which he supposed the nitrous oxide gas, then recently discovered by Dr. Priestley, to be formed, so as to consist of one particle of azote and one of oxygen, the constitution now assigned to it. The different combinations of nitrogen with oxygen are then, according to Mr. Higgins, constituted in the following atomic proportions:—

Page 171.	Nitrous oxide	1	particle nitrogen	+ 1	oxygen.
„ 14.	Nitric oxide	1	„	+ 2	„
„ 134.	Red nitrous acid	1	„	+ 3	„
„ 134.	Pale nitrous acid	1	„	+ 4	„
„ 135.	Colourless nitric acid...	1	„	+ 5	„

In the course of his work, Mr. Higgins has introduced diagrams, so constructed as to explain the phenomena of chemical decomposition by a comparison of the attractive forces of the ultimate particles.

The impartial historian of chemical science will certainly not withhold from the author of these ingenious views, the praise of uncommon sagacity; though, after a careful perusal of the entire work, he will pronounce them to be rather brilliant conceptions, hastily struck off, than the fruits of sober and sustained induction. It is evident that Mr. Higgins was guided by no fixed and uniform principle, in assigning the atomic constitution of the above compound bodies. Thus it was from the assumption, resting on an incorrect analysis of Lavoisier, that nitrous air consists of oxygen and nitrogen, in the proportion by weight of 2 of the former to 1 of the latter, that he concludes that every ultimate particle of azote must be united with two of oxygen. Again, in the passage already quoted, it is evidently because one part by weight of sulphur combines with one part by weight of oxygen, that he regards the compound, sulphurous acid, as consisting of an atom of sulphur and an atom of oxygen. By parity of reasoning he ought to have inferred water to be a compound of one atom of hydrogen with eight atoms of oxygen. Mr. Higgins's notion of the composition of the three last compounds of azote and oxygen was purely conjectural, being supported by no experimental evidence whatever.

It cannot, then, be deemed surprising that opinions, thus hastily conceived, imperfectly developed, and even inconsistent with each other, which moreover were buried under a mass of controversy of ephemeral interest, should have soon ceased to attract notice, and should not, till a later period, have come within the cognizance of one, who read so little as

Dalton. As, however, Mr. Higgins, in a subsequent work, *Experiments and Observations on the Atomic Theory*, published in 1814, written expressly to vindicate his title to be regarded as its discoverer, charges Dalton, if not directly, yet by implication, with plagiarism* (pp. 9, 10, 17, 164, &c.), and as the question of the originality of Dalton's doctrines may be raised by the future historian of science, I deem it important to perpetuate here the oral and written testimony in my possession. I have heard my father affirm, on various occasions, and to various persons, that Dalton had never seen Mr. Higgins's work, till some years subsequently to the publication of the *New System*, when it was lent to him by my father. I have also found among my father's papers the following memorandum of a conversation with Mr. P. Clare, respecting Mr. Dalton, dated February 26, 1833:—

"Mr. Clare recollects that many years ago, at the time when Mr. Ewart lived in George-street, he was invited to breakfast at Mr. Ewart's with Leslie, who was then passing through the town. The professor proposed a walk, during which Mr. Leslie told Dalton that Davy, in a paper in the *Philosophical Transactions*, had denied Dalton's claim to the atomic theory, and had set up one for Higgins. This was new to Mr. Dalton, and, on my joining the party, I declared that neither my father nor I† had received the memoir. Mr. Ewart observed that if Dalton was not the author, neither was Higgins, but that it must be given to Democritus.

It must then have been after this time that, calling on Mr. Dalton, I found him in the act of reading the note to Davy's paper in the *Phil. Trans.*, 1811, p. 15. He expressed his surprise, and asked me if I had seen Higgins's book. I

* "I cannot with propriety or delicacy directly say that Mr. Dalton is a plagiarist, although appearances are against him. Probably he never read my book; yet it appears extraordinary that a person of Mr. Dalton's industry and learning should neglect one of the few works that were expressly written on the subject of theory. At the time it was published, there were one thousand copies of it sold."

† At that period the only Fellows of the Royal Society resident in Manchester.

told him that I had not only seen it, but quoted it, and lent him the volume."*

This testimony is conclusive. It would be superfluous to those who were personally acquainted with Dalton, and who knew closely his mental habitudes and modes of study. It was never his practice to devote much of his time to reading. His was emphatically a self-reliant and productive nature; and it was by persistent efforts of thought, by direct interrogation of nature herself, and not by the study of books, that he achieved his great discoveries. What Playfair has so admirably said of Dr. Hutton may be strictly predicated of Dalton: "that the originality of his own conceptions, and the little regard he paid to authority in matters of theory, relieve us from the necessity of looking to others for the sources of his opinions." We have already traced his first general conception of atoms to the prolonged study of atmospheric phenomena, and to the contemplation, thence issuing, of matter as existing in the condition of elastic fluidity. It is now important to ascertain the steps by which he advanced from this first notion to the idea that the atoms of different bodies have different weights, and that those relative weights might be derived from the proportions by weight in which bodies chemically combine.

We have already seen that Dalton, in his earliest chemical memoir, "On the proportion of the several gases in the atmosphere" in 1802, had discovered, in the combinations of oxygen with nitrous gas, an undoubted example of multiple proportions. In his own words, "these facts clearly point out the theory of the process; the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity." I find no record, in the papers which are in my possession, of the successive steps by which he ascended from this first special example, to the general law of multiple proportions; but we have the authoritative testimony of Dr. Thomson, who spent a day or two with him in Manchester, in the month of

* This copy may now be consulted in the library of Owen's College, Manchester.

August, 1804, that the light carburetted hydrogen and olefiant gas were the next examples of the law that presented themselves to him. Dr. Thomson's words, indeed, affirm more than this. Thus—"Mr. Dalton informed me that the atomic theory first occurred to him during his investigations of olefiant gas and carburetted hydrogen gas, at that time imperfectly understood, and the constitution of which was first fully developed by Mr. Dalton himself. It was obvious from the experiments which he made upon them, that the constituents of both were carbon and hydrogen, and nothing else; he found, further, that if we reckon the carbon in each the same, then carburetted hydrogen contains exactly twice as much hydrogen as olefiant gas does. This determined him to state the ratios of these constituents in numbers, and to consider the olefiant gas a compound of one atom of carbon and one atom of hydrogen; and carburetted hydrogen of one atom of carbon and two atoms of hydrogen.* The idea, thus conceived, was applied to carbonic oxide, water, ammonia, &c., and numbers representing the atomic weights of oxygen, azote, &c., deduced from the best analytical experiments which chemistry then possessed."† This statement is confirmed by Dalton's own words in treating afterwards of carburetted hydrogen—(*New System*, vol. i., p. 444), 1810—"No correct notion of the constitution of the gas about to be described seems to have been formed till the atomic theory was introduced and applied in the investigation. It was in the summer of 1804 that I collected, at

* In a subsequent biographical account of Dalton, read before the Glasgow Philosophical Society, November 5, 1845, Dr. Thomson repeated the same statement; but in his notice of Wollaston, read November, 1850, he states: —"Mr. Dalton founded his theory on the analysis of two gases, namely, protoxide and deutoxide of azote. . . . . The first of these he considered as a compound of one atom of azote with one atom of oxygen, and the second of one atom of azote united with two atoms of oxygen." There is no doubt that the earlier statement is the correct one. For Dalton never regarded nitrous oxide as a "binary compound," but as constituted of two atoms azote and one of oxygen, and nitrous gas as one and one. See all his successive atomic tables, and his letter to Dr. Daubeny.—*Atomic Theory*, p. 477.

† *History of Chemistry*, vol. ii. p. 291.

various times and in various places, the inflammable gas obtained from ponds." He had therefore been working at the analysis of this gas just previously to Dr. Thomson's visit. Moreover, in his first table of atomic weights, (see page 59) in which hydrogen being unity, carbon was estimated 4·3; olefiant gas is represented by 5·3—that is, C + H, and carburetted hydrogen from stagnant water by 6·3, or C + 2H. This same table supplies other examples of the law of multiple proportions, which (in the absence of more direct testimony) we may reasonably presume to have constituted the foundations of that most significant generalization. Thus, carbonic oxide and carbonic acid are denoted by numbers equal to C + O and C + 2O respectively; sulphurous and sulphuric acid by numbers equal to S + O and S + 2O; and three of the nitrous compounds—nitrous oxide, nitrous gas, and nitric acid, by numbers equivalent to 2N + O, N + O and N + 2O.

There can then be no doubt, that, at the period when Dalton constructed this table, he was in full possession of the law of multiple proportions. The table is appended to his memoir on the absorption of gases, read before the Manchester Society, October 21, 1803, but it must have been added subsequently to that date, since we know from himself and Dr. Thomson that it was not before the summer of the following year, 1804, that he analysed the two carburetted hydrogens which stand last on the table. As the volume of the *Manchester Memoirs*, in which it is contained, was not published before November, 1805, the table and the last paragraph (8) of the Essay referring to it, may have been inserted at any time prior to the date of publication.

Almost all the first chemical writers of the present day concur in attributing to Dalton, without reservation, the fame of this signal discovery. Thus Berzelius affirms (*Lehrbuch*, 5 B. p. 23): "To Dalton belongs the honour of the discovery of this part of chemical proportions, which we name multiple proportions, and which none of his predecessors had observed." Dr. Hermann Kopp, author of the most recent and elaborate history of chemistry, states (B. i. p. 366)—"The discovery of multiple proportions, and the determination that the

atomic weight of a compound is given by the sum of the atomic weights of its constituents, are Dalton's *uncontested* property." Again, (B. ii. p. 370): "Dalton discovered that if a body combines in various proportions by weight with the same proportion of another body, the first proportions are simple multiples among each other; he *discovered the law of multiple proportions*." I observe, however, that my friend Professor Poggendorff, while he cheerfully recognizes Dalton's great merits, is disposed to attribute the first announcement of the law to Proust. "The law of multiple proportions was indeed first pointed out about the year 1801 by Proust, who especially defended successfully the fixity of chemical combinations against the opposite doctrine of Berthollet; but it was first raised to universality by Dalton, who is therefore not unjustly regarded as the discoverer of this law.*" But Dr. Kopp has clearly shewn that this relation had never occurred to Proust, and was in fact concealed from him by the mode in which he expressed the results of his analyses—viz., by stating the weights of the constituents in 100 parts of the compound. Thus, he expressed the constitution of the two oxides of copper and tin as under:—

	Suboxide of Copper.	Protoxide of Copper.	Suboxide of Tin.	Oxide of Tin.
Metal	86·2	80	87	78·4
Oxygen	13·8	20	13	21·6
	100	100	100	100

It is obvious that no simple relation between the weights of the oxygen in the lower and higher oxide, is derivable from comparison of the above numbers; but, if it had occurred to Proust to render the quantities of oxygen which combine with the *same weight* of the metal the objects of comparison, Dr. Kopp has pointed out† that his analyses were sufficiently exact to reveal a tolerable approximation in the oxygen of the two oxides of copper and tin respectively (13·8 : 21·5 and 13 : 24) to the simple ratio of 1 : 2.

I regret that I have not discovered among the letters and

* Article "Atomentheorie," *Handwörterbuch der Chemie*, B. i. p. 589.

† Kopp, *Geschichte der Chemie*, B. i. p. 360; B. ii. p. 370.

journals of Dalton any records, that enable me to define with greater precision than as above, the series of experimental researches and of mental processes which led him to ascertain the atomic weights of the twenty-one bodies included in his first table. I cannot even determine with absolute certainty the dates of the printed passages; for though one essay was read November, 1802, and the second October, 1803, they were both published in the same volume of the *Manchester Memoirs*, November, 1805, and may possibly have been altered so as to bring them up to the state of his knowledge at the latest date. As respects the passage describing the earliest example of multiple proportions that occurred to him, there is reason to believe that it stood as since published in the paper, when read November 1802. For I have previously quoted from a letter to his brother of March 21, 1803, that he had been for some time, and even during his Christmas vacation, working in "a track that has not been much trod in before." From a careful examination of all the evidence before me, I am led to conclude that the facts and reasonings on which the first table of atomic weights was based, were assembled by Dalton during the years 1802, 1803, and 1804,* and that the discovery of the law of multiple proportions was, in the order of mental operations, the immediate antecedent of the atomic theory of chemical combination. Thus it will be seen on inspection of the table, that of the fifteen compound atoms whose weights are assigned, not fewer than nine are examples of multiple proportions, viz., the two carburetted hydrogens, the two compounds of carbon and oxygen, the two compounds of sulphur and oxygen, and the three of oxygen and nitrogen. It is also worthy of remark, as confirming the genealogy of the atomic theory, already traced from the abstract conception of elastic fluidity, that, of the twenty-one bodies comprehended in Dalton's earliest table, sixteen are either permanent gases or vaporizable

* "In 1803, the author was gradually led to those primary Laws which seem to obtain in regard to heat and to *chemical combinations*, and which it is the object of the present work to exhibit and elucidate."—Preface to First Part of *New System*.

bodies; and that of the remaining five, Dalton calculated the atomic weights of the three solids, carbon, sulphur, and phosphorus, from the analysis of their *aëriform* combinations with hydrogen and oxygen, and those of the two liquids, sulphuric and nitric acids, from the lower *aëriform* compounds of sulphur and azote respectively with oxygen. Not a single metal, alkali, or earth, appears in this first table. The atomic weights of these *solid* bodies were first published by him in the description of Plate IV of his *New System,* Part I., p. 219, 1808.

The evidences flowing from a survey of Dalton's previous scientific labours, mainly directed upon the atmosphere, vapours, and the general relations of simple or mixed elastic fluids; from the direct testimony of so competent a witness as Dr. Thomson; from his writings in 1803 and 1804; and especially from the above critical analysis of his first published table of atomic weights, strike me as unequivocally demonstrating the genesis of the atomic theory as a general physical conception from the study of matter in the *aëriform* condition; and its first practical application in chemistry to *gaseous* bodies, and emphatically to such as combine *in multiple proportions.* But it is no less certain that Dalton's meditations on this mighty theme were materially influenced by the law of *reciprocal* proportions or equivalents of Richter. Thus in a conversation with my father, already recorded (page 63), he "confirmed the account he before gave me of the origin of his speculations, leading to the doctrine of simple multiples, and of the influence of Richter's table in exciting these views." And from my own journal, when his pupil, I copy verbatim the following passage:—"1824, February 5. The speculations which gave birth to the atomic theory were first suggested to Mr. Dalton by the experiments of Richter on the neutral salts. That chemist ascertained the quantity of any base, as potash for example, which was required to saturate 100 measures of sulphuric acid. He then determined the quantities of the different acids which were adequate to the saturation of the same quantity of potash. The weights of the other alkaline bases entering into chemical

combination with 100 parts of sulphuric acid were then obtained; and these it is obvious (?) would be equivalent to the saturation of the quantities of the different acids before determined. On these principles a table* was formed, exhibiting the proportions of the acids and the alkaline bases constituting neutral salts It immediately struck Mr. Dalton that *if these saline compounds were constituted of an atom of acid and one of alkali, the tabular numbers would express the relative weights of the ultimate atoms.* These views were confirmed and extended by a new discovery of Proust. He maintained that the compounds of iron and oxygen are strictly definite; in other words, that 100 parts of iron combine either with twenty-eight or forty-two parts of oxygen, but with no intermediate quantity. He did not, however, discover the existence of multiple proportions.† This law was first developed by Mr. Dalton, and contributed in a great degree to establish his theory of atomic proportions."

These expressions of Dalton (noted down by me immediately after each lesson), determine affirmatively the question upon which Davy‡ hesitated to pronounce judgment, viz., whether Richter's writings were known to him. They may even be thought to prove too much, and to be scarcely reconcilable with his earlier communications to Dr. Thomson, and with the view of the origin of the atomic doctrine I have endeavoured to maintain; inasmuch as Dalton assigns to the neutral salts precedence before aëriform bodies, in suggesting the principles of his new philosophy. My own belief is, that during the three years (1802—4) in which the main foundations of the atomic theory were laid, Dalton had patiently and maturely reflected on all the phenomena of chemical combination known to him, from his own researches or those of others, and had grasped in his comprehensive survey, as significant to him of a deeper meaning than to his predecessors, their empirical laws of constant and reciprocal proportion, no less than his own law of multiple proportion

* Probably that published by Richter in 1803.—See *Kopp*, B. ii. p. 366.

† Confirmed by Kopp, already quoted, B. ii. p. 370.

‡ *Six Discourses*, p. 128, quarto edition.

and his own researches in the chemistry of aëriform bodies. On reviewing in conversation, after the lapse of twenty years, the labours of the past, Dalton himself may have failed in recalling the antecedents of his great discovery in the exact order of sequence.* His fresh utterances to Dr. Thomson in 1804, when fervently engaged in the investigation, are more likely to be accurate, especially as they are confirmed by the special direction of all his previous researches. At all events it is the obvious duty of a conscientious historian to record faithfully all documents in his possession.

Besides the law of multiple proportions, Dr. George Wilson† and Dr. Hermann Kopp attribute to Dalton the discovery of the law, designated by the former writer, that of "compound proportion, which teaches, that the combining proportion of a compound body is the sum of the combining proportions of its components." Or in Dr. Kopp's‡ words, "As an important extension of stöichiometrical knowledge, for which we are indebted to Dalton, must be recorded his discovery, that the atomic weight of a compound is expressed by the sum of the atomic weights of its constituents, a result which not only flows from his atomic theory, but was also experimentally established by him." These two propositions are not, however, identical. That of Dr. Kopp is obviously involved in the very existence of the atomic hypothesis, and was, we shall see, *tacitly and indirectly* expressed by Dalton, in the statement that "1 atom of A + 1 atom of B = 1 atom of C binary, &c., &c., &c." It is indeed so essentially inter-

* This view, viz. that Dalton's acquaintance with the writings of Richter was posterior, in the order of time, to his experiments on the two carburetted hydrogens and other gases; and that those writings rather confirmed than originally suggested his atomic doctrine, is strengthened by the following decisive words of Dr. Thomson:—"I do not know when he adopted these notions, but when I visited him in 1804, at Manchester, he had adopted them; and at that time both *Mr. Dalton and myself were ignorant of what had been done by Richter on the same subject.*"—*Proceedings of the Philosophical Society of Glasgow*, 1845–46, p. 86. Again:—"Nobody knows better than myself that Dalton was ignorant of what Richter had done about ten years before him."—p. 88.

† Dr. Wilson, *British Quarterly Review*, vol. i. p. 169.

‡ Kopp's *Geschichte*, B. ii. p. 372.

woven with the primitive conception and enunciation of the atomic theory as not, in my judgment, to be entitled to rank as a separate discovery. But the law of compound *proportion*, as defined by Dr. Wilson, is, he justly affirms, "independent of any hypothesis;" in short, a rigidly empirical law. As such however, that is, as distinct from the atomic doctrine, or as expressed in any but strictly atomic language (like that employed by Kopp), I am not aware that Dalton has in any of his writings enunciated a law of "compound proportion." The truth seems to be, as a matter of history, that this law, as well as what are now termed the laws of definite and reciprocal proportion, were rather necessarily implied in the theoretical conception of Dalton, for the former, and in the empirical results of Wenzel and Richter, for the two latter, than distinctly formulized by their authors, in the shape of abstract propositions. This task has been left to the ingenuity of subsequent writers, at a more advanced stage of theoretical development. Even the law of simple multiples, Dalton's uncontested discovery, was never enunciated by him as an independent empirical truth, but was ushered into the world clothed in atomic language, as we shall now perceive, in his chapter on synthesis, which is here introduced *in extenso*.

"On Chemical Synthesis.

"When any body exists in the elastic state, its ultimate particles are separated from each other to a much greater distance than in any other state; each particle occupies the centre of a comparatively large sphere, and supports its dignity by keeping all the rest, which by their gravity, or otherwise, are disposed to encroach upon it, at a respectful distance. When we attempt to conceive the *number* of particles in an atmosphere, it is somewhat like attempting to conceive the number of stars in the universe; we are confounded with the thought. But if we limit the subject, by taking a given volume of any gas, we seem persuaded that,

let the divisions be ever so minute, the number of particles must be finite; just as in a given space of the universe, the number of stars and planets cannot be infinite.

"Chemical analysis and synthesis go no farther than to the separation of particles one from another, and to their reunion. No new creation or destruction of matter is within the reach of chemical agency. We might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen. All the changes we can produce, consist in separating particles that are in a state of cohesion or combination, and joining those that were previously at a distance.

"In all chemical investigations, it has justly been considered an important object to ascertain the relative *weights* of the simples which constitute a compound. But unfortunately the inquiry has terminated here; whereas from the relative weights in the mass, the relative weights of the ultimate particles or atoms of the bodies might have been inferred, from which their number and weight in various other compounds would appear, in order to assist and to guide future investigations, and to correct their results. Now it is one great object of this work, to shew the importance and advantage of ascertaining *the relative weight of the ultimate particles, both of simple and compound bodies, the number of simple elementary particles which constitute one compound particle, and the number of less compound particles which enter into the formation of one more compound particle.*

"If there are two bodies, A and B, which are disposed to combine, the following is the order in which the combinations may take place, beginning with the most simple, namely:

1 atom of A + 1 atom of B = 1 atom of C binary.
1 atom of A + 2 atoms of B = 1 atom of D ternary.
2 atoms of A + 1 atom of B = 1 atom of E ternary.
1 atom of A + 3 atoms of B = 1 atom of F quaternary.
3 atoms of A + 1 atom of B = 1 atom of G quaternary, &c. &c.

"The following general rules may be adopted as guides in all our investigations respecting chemical synthesis.

"1st. When only one combination of two bodies can be obtained, it must be presumed to be a *binary* one, unless some cause appear to the contrary.

"2nd. When two combinations are observed, they must be presumed to be a *binary* and a *ternary*.

"3rd. When three combinations are obtained, we may expect one to be a *binary* and the other two *ternary*.

"4th. When four combinations are observed, we should expect one *binary*, two *ternary*, and one *quaternary*, &c.

"5th. A *binary* compound should always be specifically heavier than the mere mixture of its two ingredients.

"6th. A *ternary* compound should be specifically heavier than the mixture of a binary and a simple, which would, if combined, constitute it, &c.

"7th. The above rules and observations equally apply, when two bodies, such as C and D, D and E, &c., are combined.

"From the application of these rules to the chemical facts already well ascertained, we deduce the following conclusions:—1st. That water is a binary compound of hydrogen and oxygen, and the relative weights of the two elementary atoms are as 1 : 7, nearly. 2nd. That ammonia is a binary compound of hydrogen and azote, and the relative weights of the two atoms are as 1 : 5, nearly. 3rd. That nitrous gas is a binary compound of azote and oxygen, the atoms of which weigh 5 and 7 respectively; that nitric acid is a binary or ternary compound according as it is derived, and consists of one atom of azote, and two of oxygen, together weighing 19; that nitrous oxide is a compound similar to nitric acid, and consists of one atom of oxygen and two of azote, weighing 17; that nitrous acid is a binary compound of nitric acid and nitrous gas, weighing 31; that oxynitric acid is a binary compound of nitric acid and oxygen, weighing 26. 4th. That carbonic oxide is a binary compound, consisting of one atom of charcoal, and one of oxygen, together weighing nearly 12; that carbonic acid is a ternary compound, (but sometimes binary) consisting of one atom of charcoal, and two of oxygen, weighing 19; &c., &c. In all these cases the weights

are expressed in atoms of hydrogen, each of which is denoted by unity.

"In the sequel, the facts and experiments from which these conclusions are derived, will be detailed; as well as a great variety of others from which are inferred the constitution and weight of the ultimate particles of the principal acids, the alkalis, the earths, the metals, the metallic oxides and sulphurets, the long train of neutral salts, and, in short, all the chemical compounds which have hitherto obtained a tolerably good analysis. Several of the conclusions will be supported by original experiments.

"From the novelty as well as importance of the ideas suggested in this chapter, it is deemed expedient to give plates, exhibiting the mode of combination in some of the more simple cases. A specimen of these accompanies this first part. The elements or atoms of such bodies as are conceived at present to be simple, are denoted by a small circle, with some distinctive mark; and the combinations consist in the juxta-position of two or more of these; when three or more particles of elastic fluids are combined together in one, it is to be supposed that the particles of the same kind repel each other, and therefore take their stations accordingly."

The detailed experimental evidence followed in the second part of the first volume of the *New System.*

It must be conceded, *in limine,* that the great atomic generalization does not stand on the solid basis of induction, which is here, in its first announcement, claimed for it, by its illustrious author. Even assuming the existence of elementary atoms of different weights (itself obviously hypothetical), we are not in possession of the mathematical elements necessary to infer "from the relative weights in the mass, the relative weights of the ultimate particles or atoms of the bodies." All that is *certainly* established, is, the proportions by weight, in which bodies combine,—in Dalton's words, "the relative weights of the simples, which constitute

a compound." Now these relative or equivalent weights are, on the atomic hypothesis, the products of the atomic weights into the number of atoms. Consequently, to infer the unknown atomic weights from the known equivalents, we require the number of atoms. But these numbers cannot be *ascertained* in the case of a single chemical compound. They can only be gathered according to grounds of *probability* from various relations and arguments, to be hereafter enumerated. This general statement will be made clear by a single example. It is established by experiment that 8 parts by weight of oxygen combine with 1 part by weight of hydrogen. Let W and w represent the unknown atomic weights of oxygen and hydrogen, and N and n the numbers of atoms present in each mass respectively.

$$\text{Then } NW = 8 \text{ and } nw = 1$$
$$\text{And } \quad W = \frac{8}{N} \text{ and } w = \frac{1}{n}$$

W and w cannot be inferred without knowing N and n.

Mr. Dalton derived the numerical values of N and n from a supposition, which, though highly probable, is still nothing more than an hypothesis. He maintained, that the most stable combinations must be binary, and that when only a single combination of two elements was known, it was probably binary. Water was the only compound of oxygen and hydrogen then known to exist. He therefore regarded it as constituted of one atom of oxygen and one atom of hydrogen. Hence N and n being identical, the atomic weights will be represented by the equivalents 8 and 1.

$$\text{For } W : w :: \frac{8}{N} : \frac{1}{n} :: 8 : 1$$

To bring out more distinctly the hypothetical character of this reasoning, it may be here observed, that Berzelius never admitted water to be a "binary compound;" but inferred from its being constituted of 1 part by *volume* of oxygen and 2 parts of hydrogen by *volume*, that it consists of 1 atom of oxygen united with 2 atoms of hydrogen. Substituting

1 and 2 for N and n in the above formula, we obtain 16 as the weight of the atom of oxygen, hydrogen being unity.

Mathematical certainty is, therefore, not attainable (even if we admit the existence of atoms of different weights), in any single atomic determination. It is vain to contend for a higher degree of evidence than that of *probabilities*, in support of the atomic hypothesis itself; or still less of special calculations of the weights of elementary and compound atoms.

After this distinct avowal, it will be more satisfactory to recal Dalton's mode of estimating the number of atoms in compounds, in his own words, as expressed in the appendix to his second volume, 1827, and as therefore conveying his matured views and final teaching.

"The second object of the atomic theory, namely, that of investigating the *number* of atoms in the respective compounds, appears to me to have been little understood, even by some who have undertaken to expound the principles of the theory. When two bodies, A and B, combine in multiple proportions; for instance, 10 parts of A combined with 7 of B to form one compound, and with 14 to form another, we are directed by some authors to take the smallest combining proportion of one body as representative of the elementary particle or atom of that body. Now it must be obvious to any one of common reflection, that such a rule will be more frequently wrong than right. For, by the same rule, we must consider the first of the combinations as containing 1 atom of B, and the second as containing 2 atoms of B, with 1 atom or more of A; whereas it is equally probable by the same rule, that the compounds may be 2 atoms of A to 1 of B, and 1 atom of A to 1 of B respectively; for, the proportions being 10 A to 7 B (or, which is the same ratio, 20 A to 14 B), and 10 A to 14 B, it is clear by the rule, that when the numbers are thus stated, we must consider the former combination as composed of 2 atoms of A, and the latter of 1 atom of A, united to 1 or more of B. Thus there would be an *equal* chance for right or wrong. But it is

possible that 10 of A and 7 of B, may correspond to 1 atom A, and 2 atoms B; and then 10 of A, and 14 of B, must represent 1 atom A, and 4 atoms B. Thus it appears the rule will be more frequently wrong than right. It is necessary not only to consider the combinations of A with B, but also those of A with C, D, E, &c., as well as those of B with C, D, E, &c., before we can have good reason to be satisfied with our determinations as to the *number* of atoms which enter into the various compounds. Elements formed of azote and oxygen appear to contain portions of oxygen, as the numbers 1, 2, 3, 4, 5 successively, so as to make it highly improbable that the combinations can be effected in any other than one of two ways. But in deciding which of those two we ought to adopt, we have to examine not only the compositions and decompositions of the several compounds of these two elements, but also compounds which each of them forms with other bodies. I have spent much time and labour upon these compounds, and upon others of the primary elements, carbon, hydrogen, oxygen, and azote, which appear to me to be of the greatest importance in the atomic system; but it will be seen that I am not satisfied on this head, either by my own labour or that of others, chiefly through the want of an accurate knowledge of combining proportions."

To Dr. Thomson, of Glasgow, must be awarded the honour of first embracing and making known to the world the atomic philosophy. It was during his visit to Manchester, in 1804, already mentioned, that he learned from Dalton's lips, his new doctrine, and the experimental evidence on which it reposed. Dr. Thomson saw at a glance the immense importance of this theory, and at once cordially adopting it, became and continued, during a long and brilliant scientific career, its most earnest and persevering expounder. He first announced its principles in the third edition of his *System of Chemistry*, p. 424, &c.; and in January 1808, brought it prominently, with praiseworthy hardihood, before the notice of the Royal Society. In this memoir, on oxalic acid, he showed the existence of two salts

of oxalic acid and potash, the oxalate and superoxalate, in the last of which the acid was found to be "very nearly double what is contained in the oxalate." He also proved that there are two oxalates of strontian, and "that the first contains just double the proportion of base contained in the second." These remarkable examples of the law of multiple proportions constituted, of themselves, especially at the time when they were made known, invaluable facts in favour of the atomic theory. But Dr. Thomson ventured further, at the close of his memoir,* to lay down distinctly and fully the doctrines of Dalton, and to give the atomic weights of several bodies; all, it may be observed, in the *gaseous state*, which Dalton had then obtained. "This curious theory," he observes, "which promises to throw an unexpected light on the obscurest parts of chemistry, belongs to Mr. Dalton." It required both a very profound conviction of the truth of the new doctrine, then in its earliest stage of development, and supported by no considerable amount of experimental evidence, and that indomitable courage in avowing his convictions, which was a characteristic attribute of Thomson, to prompt him thus to stand forth its single acknowledged convert and champion.†

At the next succeeding meeting of the Royal Society, Dr. Wollaston read his remarkable memoir on superacid and subacid salts. In this he points out the existence of the law of simple multiples in the subcarbonate and carbonate of potash and soda, in the supersulphate and sulphate of potash, and in the three compounds of potash and oxalic acid, the oxalate, binoxalate, and quadroxalate. In these last the weights of acid combining with a constant quantity of base are represented by the numbers 1, 2, and 4. He regards these facts as "but particular instances of the more general observation of Mr. Dalton, that in all cases the simple elements of bodies are disposed to unite atom to atom singly,

* *Phil. Trans.* 1808, p. 86, &c.

† For a further account of Dr. Thomson's services in teaching and promulgating the atomic philosophy, see Mr. Walter Crum's "Biographical Notice of Thomson," *Proceedings of the Philos. Soc. of Glasgow*, 1852–53.

or if either is in excess, it exceeds by a ratio to be expressed by some simple multiple of the number of its atoms." He adds: "I am further inclined to think, that when our views are sufficiently extended to enable us to reason with precision concerning the proportions of elementary atoms, we shall find the arithmetical relation alone will not be sufficient to explain their mutual action, and that we shall be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension."* Indeed the atomic philosophy, as holding out the promise of a degree of precision then deemed unattainable in chemical analysis, and as founded on strict dynamical principles, must have peculiarly attracted the acute and exact mind of Wollaston, nurtured in the severe discipline of the pure mathematics, and always turning with marked predilection to those branches of physics most susceptible of mathematical expression and reasoning.† Nor could the unreserved assent of so cautious a thinker, to the doctrine of atoms, fail to hasten its general reception among men of science. Davy, the other great coeval leader, for some time stood aloof, finding in the Daltonian philosophy no congenial element that he could accept and assimilate. Their minds and cast of genius were indeed strongly contrasted. "Bold, ardent, and enthusiastic, Davy soared to loftier heights; he commanded a wider horizon: and his keen vision penetrated to its utmost boundaries. His imagination, in the highest degree fertile and inventive, took a rapid and extensive range in pursuit of conjectural analogies, which he submitted to close and patient comparison with known facts, and tried by an appeal to ingenious and conclusive experiments. He was imbued with the spirit, and was a master in the practice, of the inductive logic: and he has left us some of the noblest examples of the efficacy of that great instrument of human reason in the discovery of truth. He applied

* *Phil. Trans.* 1808, p. 101.

† Dr. Thomson always said that, in the absence of Dalton, Wollaston would have been, very soon, the discoverer of the atomic theory.—Mr. W. Crum, *Proc. of Phil. Soc. of Glasgow*, 1852–53, page 257.

it, not only to connect classes of facts of more limited extent and importance, but to develope great and comprehensive laws, which embrace phenomena that are almost universal to the natural world. In explaining those laws, he cast upon them the illumination of his own clear and vivid conceptions;—he felt an intense admiration of the beauty, order, and harmony, which are conspicuous in the perfect CHEMISTRY OF NATURE;—and he expressed those feelings with a force of eloquence, which could issue only from a mind of the highest powers, and of the finest sensibilities."*

In a letter dated Nov. 15, 1809, in which Davy announces to Dalton, that the managers of the Royal Institution accept his proposal to deliver a second course of twenty lectures on natural philosophy, heat, and *elementary principles*, Davy adds: "I shall be very glad to hear your new views of the atomic system. I think it likely that there is always a regular order of proportions in composition, but I doubt whether we have yet obtained any elements; and I am convinced that there are yet great changes to be made in our existing arrangements." In the year 1811 Davy thus expressed his matured objections to the doctrines of Dalton: "I shall enter no further at present into an examination of the opinions, results, and conclusions of my learned friend; I am, however, obliged to dissent from most of them, and to protest against the interpretations that he has been pleased to make of my experiments; and I trust to his judgment and candour for a correction of his views. . . . . It is impossible not to admire the ingenuity and talent with which Mr. Dalton has arranged, combined, weighed, measured and figured his atoms; but it is not, I conceive, on any speculations upon the ultimate particles of matter, that the true theory of definite proportions must ultimately rest."† Sir

* Preface to the last edition of Dr. Henry's *Chemistry*, 1829.

† *Phil. Trans.* 1811, p. 17, note. An interesting narrative is related by Dr. Thomson, in his *History of Chemistry*, of a conversation, in the autumn of 1807, between himself, Wollaston, and Davy, on the atomic theory, when Wollaston and Thomson tried, but unsuccessfully, to make a convert of Davy. Dr. Wollaston succeeded shortly afterwards in bringing over Mr. Davies Gilbert, who is said by Dr. Thomson to have soon convinced Sir H. Davy of

H. Davy, in his work on chemical philosophy, refused to employ the term "atom," preferring that of "proportion," as a simple expression of fact, and involving no hypothesis.

In a letter to Mr. Johns, dated London, Dec. 27, 1809, Dalton, after mentioning a long discussion with Davy, in presence of Sir J. Sebright, says: "Davy is coming very fast into my views on chemical subjects." I am not aware that Sir H. Davy ever gave his absolute assent to the atomic doctrine. But in a conversation with Laplace, in November 1813, he seems more favourably inclined towards it. "On my speaking to him of the atomic theory in chemistry, and expressing my belief that the science would ultimately be referred to mathematical laws, similar to those which he had so profoundly and successfully established with respect to the mechanical properties of matter, he treated my idea in a tone bordering on contempt, as if angry that any results in chemistry could, even in their future possibilities, be compared with his own labours. When I dined with him, in 1820, he discussed the same opinion with acumen and candour, and allowed all the merit of John Dalton."—Dr. Davy's *Life of Sir H. Davy* (p. 168). In his award of the royal medal to Dalton, 1826, we shall afterwards find Sir Humphry accurately defining and warmly applauding the services of Dalton.

In 1809, the year succeeding the publication of the *New System*, Gay-Lussac's important memoir on the law of combination of the gases furnished strong support to the atomic doctrine in the judgment of most men of science, though not in that of its author. Indeed, for many years afterwards, Dalton, confiding in his own less accurate experiments, persisted in withholding his assent from the beautiful law of combination by volume in equal or multiple proportions, established by Gay-Lussac. I am not aware that he ever heartily and unreservedly accepted it. It is certainly true that the atomic theory is based upon the proportions in which bodies combine by *weight*, and that Gay-Lussac's law

the truth of the atomic doctrine. If this were the case in 1807, Davy had relapsed into a state of unbelief in 1811.

of volumes is neither derivable as a corollary from the principles of Dalton nor essential to their truth. The gases might have combined, as far as those principles are involved, in proportions by *volume*, denoted by complex fractions. Still it seems unaccountable that Dalton should not have welcomed the precise experimental results of Gay-Lussac, as supplying a body of special examples of the laws of definite and multiple proportions, and as thus ministering—like the parallel results of Wollaston on the salts—the strongest practical confirmation of his doctrines. For, in the words of Berzelius,* "if we substitute the term atom for volume, and contemplate bodies in the solid instead of the gaseous state, we find in the discovery of Gay-Lussac one of the most immediate proofs in favour of the hypothesis of Dalton."

Dr. Hermann Kopp has attempted, but I think unsuccessfully, to explain Dalton's hostility to the law of volumes on the following grounds. He conceives that in consequence of the imperfect knowledge of the specific gravities of the gases prevailing at that period, "the results of Gay-Lussac appeared at first irreconcilable with those of Dalton." For from Gay Lussac's experiments "no other conclusion is deducible than that a volume of a gas represents also its atomic weight; that the weights of equal volumes of the different gases are proportioned to their atomic weights; that consequently the specific gravities of the gases are also their atomic weights, or stand in a simple relation to them. And if this be not the case, either Dalton's or Gay Lussac's law must be incorrect. Can we be surprised in these circumstances if Dalton, satisfied of the truth of his views, were reluctant to admit the conclusions of Gay Lussac?" *Gesch. der Chemie*, t. ii. p. 379. It strikes me, that upon Dr. Kopp's own showing, the question at issue between Dalton and Gay-Lussac affected merely the numerical weights of certain individual atoms, and did not approach at all the general principles or laws of the atomic philosophy. There was no incompatibility between the law of combination by *volume* and that by *weight*. The simple relation discovered by Gay-Lussac was independent

* *Lehrbuch*, B. v. p. 23.

of, and supplemental to, that revealed by Dalton. It was so treated by Dr. Prout in his masterly essay (*Ann. of Phil.*, vol. vi., 1815), and has been since universally thus regarded. Moreover, Dalton himself, in determining the important atomic weight of water (vol. i. p. 275) bases his calculation on the experiments of Humboldt and Gay-Lussac, and concedes the fact (though at a later period he doubted it) that two measures of hydrogen require just one of oxygen to saturate them. See also his remarks on the compounds of oxygen and azote, p. 317.

No European chemist has contributed so largely to establish the atomic theory on the solid basis of multiplied and exact experiments, as Berzelius. His researches, as he has himself informed us,* took their departure, in 1807, from those of Richter, whose writings, then little esteemed, he had occasion to consult, in preparing the first edition of his system of chemistry. He was astonished at the light cast by Richter upon the constitution of salts, and the mutual precipitation of the metals; and was particularly impressed with the productive value of Richter's inference, that from the careful analysis of a few salts, the composition of all others might be obtained by calculation. He immediately undertook an extensive series of analyses of salts, conducted with a rigid precision and vigilance, then unexampled in chemistry. While engaged in these investigations, he received intelligence of Dalton's discovery of the law of multiple proportions, and of his atomic theory, and found among the results of analyses already completed, many remarkable confirmations of Dalton's doctrines. Berzelius did not, however, remain satisfied with a limited range of experimental proofs. It became the occupation of his long and glorious life to multiply these proofs by laborious analytical researches in every province of chemical science, in organic as well as inorganic chemistry. He laboured with signal success in determining the atomic weights of all chemical elements and compounds; and the numbers he finally assigned have long been almost universally accepted

* Berzelius, *Lehrbuch der Chemie*, B. v. p. 24.

by chemists, and have only lately received slight corrections from the improved methods of the present day. I have found in Dalton's repositories the following interesting letter from the illustrious Swedish chemist.

London, October 13, 181 .

Sir,—Many thanks for your obliging letter, to which I ought to have replied long since, but various little excursions in the neighbourhood of London have prevented my undertaking anything. I am now on the point of leaving, and I have to beg of you to honour me with your communications, even when I shall have returned to my country. I will not fail to impart to you all that can be interesting in sending you.

The observation which you have made on the subsulphate of the oxide of iron is perfectly well founded, as I perceived from the inaccuracy of the analyses when I found the law, according to which the sulphuric acid combines with the saline bases. You will find a detailed discussion on this matter in the *Annales de Chimie,* in the part of my last treatise, where I have spoken of the salts in excess of the base. In the real subsulphate, the acid is combined with six times as much oxide of iron as in the sulphate neuter. You will see also in this place what a singular substance the yellow powder was, which I took for pure subsulphate in my first treatise. You call the sulphate of oxide of common iron a supersulphate. I cannot see the reason of it, because the acid neutralizes a quantity of the base of which the oxygen is $\frac{1}{3}$ of that of the acid, just as in the neutral sulphate of potash. Your opinion that minium is a combination of the black oxide with the yellow oxide is perhaps founded on the difficulty of conceiving a half atom: I think that we must let experiment mature the theory; if this begin to apply itself to force nature into its forms, it will cease to be useful and to improve. *You are right in this, that the theory of multiple proportions is a mystery without the atomic hypothesis:* and as far as I have been able to see, all the results gained hitherto contribute to justify this hypothesis. I think, however, that there are parts of this

theory, much as science owes to you at present, which demand a little alteration. That part, for example, which obliges you to declare as inaccurate the experiments of Gay-Lussac, on the volumes of the gases which combine. I should have thought rather that these experiments were the finest proofs of the probability of the atomic theory; and I confess to you, that I do not so easily think Gay-Lussac in fault, especially in a matter where the point is about measuring good or bad. But the paper bids me to finish.

If you favour me with your letters, my address will be after this, Stockholm, without any further particular address. I set out to-morrow evening by Hamburgh to Gottenburg.

May God bless you.

J. Berzelius.

CHAPTER V.

ATOMIC THEORY, CONTINUED — CONFIRMATION BY SUBSEQUENT DISCOVERIES — ISOMORPHISM, ELECTROLYSIS, SPECIFIC HEAT — EQUALITY OF ATOMS IN GASES—ISOMERISM—DR. WOLLASTON'S ARGUMENT FROM THE FINITE EXTENT OF THE ATMOSPHERE—OBJECTIONS CONSIDERED—TABLE OF ATOMIC WEIGHTS — RELATION TO THAT OF HYDROGEN — OTHER NUMERICAL RELATIONS — ATOMIC DIAMETERS CALCULATED BY DALTON—ATOMIC VOLUMES—KOPP AND SCHROEDER—DALTON'S SYMBOLS—ORGANIC CHEMISTRY—RATIONAL FORMULÆ —LAW OF SUBSTITUTION—CHEMICAL TYPES—LAW OF HOMOLOGUES—ATOMIC SPIRIT OF DR. HOFMANN'S LECTURES—OPINIONS OF BERZELIUS, MITSCHERLICH, GRAHAM, GMELIN, AND HERSCHEL—LETTERS FROM FARADAY AND LIEBIG.

In the interval of half a century, which has now elapsed since the publication of Dalton's first table of atomic weights, the progress of science has presented numerous striking facts and analogies in support of the general doctrine of atoms, as well as new methods of determining N, or the *number* of atoms constituting individual compounds.

I. Among discoveries of this twofold import, that of the law of Isomorphism, by Mitscherlich, is doubtless, beyond comparison, the most important. This law, in its extreme generality, has been thus expressed: "The same number of atoms, combined in the same way, produce the same crystalline form, and crystalline form is independent of the chemical nature of the atoms, and determined only by their number and relative position."* Thus arsenic acid AsO_5, and phosphoric acid PO_5, when combined with potash, soda, ammonia, baryta, and oxide of lead, in the same degree of saturation, and with an equal number of atoms of water, form two series of salts, in which the corresponding terms possess the same crystalline

* Graham, vol. i. p. 160.

form.* The same identity of form was observed in the similar salts of selenic and sulphuric acids, and of perchloric and periodic acids,—pairs of acids regarded on chemical grounds as possessing the same atomic composition. Similar results were obtained for bases, which have been therefore ranged in isomorphous groups. It was further shewn that acids and bases belonging to the same class were capable of replacing one another in salts and minerals without causing change of form. "The fundamental idea of isomorphism is, in point of fact, a very simple one. A chemical compound may be compared to a building of definite form, in the erection of which the shape and size of the stones, not their quality, are to be regarded. Stones of the same shape, but of different material, may be exchanged for one another without altering the contour of the building. In like manner, protoxide of iron, Fe O, may be replaced by protoxide of manganese, Mn O, or potassium by sodium, in a chemical combination, without change of form." This exchange of atom for atom with persistence of the crystalline form has been termed "monomeric isomorphism."† It has been well observed by Dr. Scheerer, the author of the two elaborate articles above quoted, that if "we wish to form a clear conception of a chemical compound, and refuse to take refuge in an incomprehensible mutual interpenetration of matter, nothing remains but to admit matter, as chemically effective, to be constituted of atoms (particles or molecules) mechanically aggregated together. Indeed, the law of isomorphism (subject, it is true, to certain exceptions to be presently stated) is in its very essence *atomic,* since it affirms identity of crystalline form to be dependent on similarity of atomic composition. The fact that groups of bodies, which had been classed together as of similar composition, on purely chemical grounds, are now discovered to be also characterized by the remarkable common property of giving birth to isomorphous salts, surely furnishes strong presumption in favour of the soundness of those chemical principles of classification.

* "Isomorphismus," *Handwörterbuch,* B. iv. p. 150.

† "Isomorphismus Polymerer," B. iv. p. 170.

Certain deviations from *exact* conformity to the law, as small differences in the magnitude of the angles of crystals nearly isomorphous, and hence termed plesiomorphous or homoiomorphous, have been traced by the recent labours of Dr. Kopp to differences in the atomic volumes of the bodies compared. Three conditions* are found necessary to perfect isomorphism—viz., the same stöichiometrical formula, the same crystalline form and the same atomic volume.† Thus phosphorus and arsenic, among Mitscherlich's early examples of isomorphism,—in the salts formed by their acids,—possess different atomic volumes, and are only homoiomorphous.‡ Hence, even the departures from the strict law lend support to the doctrine of Dalton; for, in Liebig's words, "One of the most cogent arguments for the soundness of our views respecting the existence of atoms is, that these deviations are explicable upon certain considerations attaching to the atomic theory."

Isomorphism renders valuable aid in determining the number of atoms in compounds. It would constitute, indeed, an infallible rule, (that is, *assuming* the atomic composition of any *one* base and acid in each group, forming isomorphous compounds) provided the same crystalline form is invariably the expression of the same atomic constitution. But, though the prevalent rule, there exist exceptions to its universality which have not hitherto been explained. Thus RO. XO_2 is isomorphous with RO. X_2O_5; that is, certain carbonates are isomorphous with certain nitrates; and RO.XO_3 is isomorphous with RO. X_2O_7,§—that is, certain sulphates are isomorphous with certain hyperchlorates and hypermanganates. There are also the numerous cases now classed under the term "polymeric isomorphism," in which atom is not replaced by atom, but m atoms are replaced by n atoms. Thus in certain isomorphous minerals, of which cordierite and aspasiolite are examples, *one* atom of various protoxides is replaced by *three* atoms of water. Dr. Scheerer concludes, "that substances of different atomic volume, and at the same time different

* "Isomorphismus," p. 162.

† The mode of determining atomic volumes will be afterwards fully considered.

‡ "Isomorphismus," p. 167. § "Isomorphismus," p. 160.

atomic composition, are found to possess the same, or approximatively the same, crystalline forms, and that this especially occurs in the case when their atomic volumes stand in a simple multiple relation to one another."* But though not an universal and peremptory rule, isomorphism may be advantageously employed, in aid of other physical and purely chemical principles, to determine the value of N in our formula; from which determination, as being essentially a calculus of probabilities, no element can be safely eliminated.

Professor Graham thus examines the question, whether isomorphism may serve unconditionally as a means of determining the atomic weight. "Chemists have always been most anxious to possess a simple physical character by which atoms might be recognized: and equality of volume in the gaseous state, equality of specific heat, and similarity in crystalline form, have all in their turn been upheld as affording a certain criterion. The indications of isomorphism certainly accord much better than those of the other two criteria with views of the constitution of bodies derived from considerations purely chemical; and are indeed invaluable in establishing analogy of composition in a class of bodies, by supplying a precise character which can be expressed in numbers, instead of that general and ill-defined resemblance between allied bodies, which chemists perceived by an acquired tact rather than by any rule, and which was heretofore their only guide in classification. Admitting that isomorphism is a certain proof of similarity of atomic constitution, within a class of elements and their compounds, it may still be doubted whether the relation of the atom to crystalline form is the same without modification throughout the whole series of the elements, or whether all atoms agree exactly in this or any other physical character. . . . . Crystalline form then may possibly depend upon some at present unknown property of bodies, which may have a frequent and general, but certainly not an invariable, relation to their atomic constitution."—*Graham's Elements of Chemistry*, vol. i. pp. 175-6.

* "Isomorphismus," p. 170.

II. Dr. Faraday's great discovery, announced in the seventh series of his experimental researches, "that electrolytes must consist of two ions;" "that there is but one electrolyte, composed of the same elementary ions, at least such appears to be the fact, dependent upon a law, that only single electro-chemical equivalents of elementary ions; can go to the electrodes, and not multiples;" and finally, "that electro-chemical equivalents coincide, and are the same with ordinary chemical equivalents," throws considerable light upon atomic determinations. If this law be regarded as universally true, a body experimentally proved to be an electrolyte, is at once known to be a binary compound, or to consist of a single atom of each of its constituents. Water is an electrolyte, and must therefore consist of an atom of oxygen and an atom of hydrogen. Dr. Faraday himself has recognized this application of his law of electrolysis:— (851) "A very valuable use of electro-chemical equivalents will be to decide, in cases of doubt, what is the true chemical equivalent, or definite proportional or atomic number of a body; for I have such a conviction that the power which governs electro-decomposition and ordinary chemical attractions is the same, and such confidence in the over-ruling influence of those natural laws which render the former definite, as to feel no hesitation in believing that the latter must submit to them also. Such being the case, I can have no doubt, that assuming hydrogen as 1, and dismissing small fractions for the simplicity of expression, the equivalent number or atomic weight of oxygen is 8, of chlorine 36, of bromine 78·4, of lead 103·5, of tin 59, &c., notwithstanding that a very high authority doubles several of the numbers." In addition to this special application, and far surpassing it in importance, from its immediate bearing on the general question of the existence of atoms, is Dr. Faraday's great discovery, "that the equivalent weights of bodies are simply those quantities of them, which contain equal quantities of electricity . . . . . Or, if we adopt the atomic theory or phraseology, then the atoms of bodies have equal quantities of electricity naturally associated with them." Dr.

Faraday, it is true, adds, "But I must confess, I am jealous of the term atom; for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature, especially when compound bodies are under consideration." The disciples of Dalton may, however, it strikes me, fairly contend that the fact of equal quantities of electricity being associated with precisely those equivalent weights of different bodies, which had been obtained by purely chemical methods, ministers strong confirmation to the atomic hypothesis.

III. A similar relation between the atomic weights of bodies and their *specific heats*, first conjectured by Dalton to exist in elastic fluids, and subsequently established by Petit and Dulong, and Regnault, has also been appealed to in order to confirm or correct atomic denominations, based on other grounds. It has, however, been already shown that there exist certain exceptions to the universality of this law;* and even in those bodies that conform to it the products of their specific heats into their atomic weights differ more or less from a constant quantity. Moreover, to admit its rigorous application to the determining atomic weights, it must be assumed, that the absolute heats are faithfully represented by the specific heats, an assumption obviously inconsistent with Petit and Dulong's experiments, proving increased capacity for heat in the same body at higher temperatures.

IV. I have purposely avoided including among the indirect means of inferring the number of atoms entering into combination, the hypothesis, that equal volumes of the permanent gases and of vaporized bodies contain the same number of atoms, though resting on the weighty authority of Prout, Avogadro, and Dumas. For this hypothesis involves, as respects the simple gases, the necessity of regarding water as a compound of two atoms of hydrogen and one of oxygen,—a view hostile to the Daltonian canon, based upon the principles of dynamics, that the most stable compound of two elements must be binary, as well as to Dr. Faraday's law of electrolysis, according to which, water, as an electrolyte, must be a binary compound. As respects the formation of

* *Gmelin*, p. 244, and above, p. 68.

many of the compound gases, as nitrous gas and muriatic acid gas, it involves the further hypothesis, that the self-repulsive molecule, as it exists in the gaseous form, does not represent the ultimate molecule, but is composed of many of them;—an hypothesis which I have elsewhere* endeavoured to prove to be *not* deducible, as affirmed by its authors, from the law of Mariotte and from that of equal expansibility by heat, and to be irreconcilable with the postulates employed by Laplace as the basis of his profound mathematical inquiries into the constitution of elastic fluids. Finally, of vaporized bodies which cannot, on any scientific principle, be classed in a distinct category from permanently elastic fluids, the experiments of both Dumas and Mitscherlich have shown that equality of atoms in a given space cannot be predicated. The weight of the vapour of mercury was ascertained by them to be one-half, that of phosphorus vapour twice, and that of the vapour of sulphur three times as great as it should have been on calculation founded on the atomic weights derived from their chemical combinations. Mitscherlich therefore, rejects the hypothesis of equality of atoms as untenable, at all events in the compound gases, and affirms† as the result of all experiments then instituted, "that in an equal space the number of atoms in all gases stands in a simple relation." More recently Gmelin‡ has constructed a table of atomic numbers of the elementary gases and vapours, from which it appears that elementary bodies in the gaseous state contain, in a given volume, either $1x$, $2x$, or $6x$ atoms, and may, therefore, be divided into hexatomic, diatomic, and monatomic gases. In compound elastic fluids we also meet with $\frac{2}{3}$, $\frac{1}{2}$, $\frac{1}{3}$, and $\frac{1}{4}$ atomic numbers."§

These four are the principal laws or analogies which have been appealed to as subsidiary or supplemental to those propounded by Dalton, in determining the *number* of atoms in

* Remarks on the Atomic Constitution of Elastic Fluids, *Phil. Mag.* 1834.

† *Ueber das Verhältniss des spec. Gewichts der Gasarten zu den chemischen Proportionen*, pp. 2, 3.

‡ Gmelin, *Handbook*, vol. i. p. 53.

§ See also *infra*, Kopp's Table of the Atomic Volumes of Gases, of which the atomic numbers are the reciprocals.

compounds. Other analogies have been since disclosed, and still reveal themselves in exact parity with the onward march of science, which swell the weight of probabilities in support of the general doctrine of the atomic constitution of matter. Among such, a conspicuous station is due to the phenomena that have been classed under the general term Isomerism. Isomeric bodies, in the primitive and most comprehensive acceptation of the word, are such as are composed of the same elements, in the same proportions, but yet exhibit totally distinct properties. Thus, chemical analysis discloses the same per-centage of carbon and hydrogen (86 and 14 nearly) in bodies so dissimilar as olefiant gas, gas from oil, naphtha, and cetene. Among organic compounds* are found a vast number of wholly dissimilar bodies, which, like those just mentioned, contain the same elements, carbon and hydrogen, in the same proportions. Berzelius,† by whom the general doctrine of isomerism, based upon the facts observed by Wöhler, Liebig, Faraday, and Clark, was first propounded, has distinguished four classes of isomeric bodies:—1. Bodies of the same atomic weight, in which a difference of rational composition cannot be assumed, but in which a difference only in the mode of arrangement of their elementary atoms may be conjectured to exist.—2. Combinations, which differ only in containing the same constituent in different allotropic conditions.—3. Polymeric compounds, as the four hydrocarbons already enumerated, C^4H^4; C^8H^8; $C^{16}H^{16}$; $C^{32}H^{32}$, consisting of the same elements in the same proportions per cent; but having *different* atomic weights.—4. Metameric compounds, or such as have the *same* atomic weight, but are formed by different proximate constituents; as,

Formiate of oxide‡ of ethyl, $C^4H^5O + C^2HO^3$
Acetate of oxide of methyl, $C^2H^3O + C^4H^3O^3$

which are both alike represented by the empirical formula, $C^6H^6O^4$.

The existence of isomeric bodies, such as are defined

* Liebig's *Letters*, p. 160.
† Kopp, "Isomeric," *Handwörterbuch*, B. iv. pp. 142, 146, 148.
‡ Graham's *Chemistry*, vol. i. p. 183.

under the first category, viz., identical in atomic weights as well as in per-centage proportion and in proximate constitution; such, in short, as are neither polymeric nor metameric, has been shown by Professor Graham to be exceedingly doubtful. "Isomeric bodies have in general been proved by the progress of discovery to agree in the relative proportion of their constituents only, and to differ either in the aggregate number of the atoms composing them (polymerism), or in the mode of arrangement of their atoms (metamerism); and although new cases of isomerism are constantly arising, others are removed as they come to admit of explanation." Eliminating, however, such equivocal examples, the numerous undoubted cases of polymeric and metameric compounds suffice for my present purpose, which is to show the inadequacy of the empirical law of combining proportions to account for the striking diversities in physical and chemical properties of bodies, constituted of the same elements in the same proportions; and the clear disclosure of the nature of these compounds, derived from contemplating the weights and modes of grouping of their constituent atoms. In the decisive words of Baron Liebig,* "the doctrine that matter is not infinitely divisible, but, on the contrary, consists of atoms incapable of further division, alone furnishes us with a satisfactory explanation of these phenomena."

It seems expedient not to pass unnoticed Dr. Wollaston's celebrated argument, in support of the atomic hypothesis, though its soundness has been impeached by the highest authority.† He has urged, that if matter be infinitely divisible, there can exist no limit to the earth's atmosphere, "which must pervade all space, and be condensed around the different planetary bodies in degrees dependent on their respective attractions." Dr. Wollaston concludes from his own and Captain Kater's observations on the passage of Venus near the sun in superior conjunction, and from the common phenomena observable in the occultation of Jupiter's

* *Letters on Chemistry*, p. 162, 3rd ed.

† Dr. Whewell, *British Association Reports*, vol. viii. p. 26.

satellites by the body of the planet, that there seems no ground whatever to admit the existence of an atmosphere around the Sun or Jupiter; and that "all the phenomena accord entirely with the supposition, that the earth's atmosphere is of finite extent, limited by the weight of ultimate atoms of definite magnitude no longer divisible by repulsion of their parts."—*Phil. Trans.* 1822, p. 89.

Dr. Whewell's objection, in the only brief notice I have seen of it, rests on mathematical grounds. As chemists, we could only bow before so weighty an authority, did we not find Laplace maintaining the molecular theory, and demonstrating that the law of Mariotte and that of equal expansibility by heat are mathematically derivable from the following suppositions:—that the molecules of gases are at such a distance that their mutual attractions are insensible; that these molecules retain caloric by a principle of attraction; that their mutual repulsion is due to the repulsion of the molecules of caloric; and finally, that this repulsion is only sensible at imperceptible distances.—*Mécanique Céleste,* livre xii. ch. 1; tome v. p. 89—91. "Sir Isaac Newton* also has shown, in the Prop. 23, Book 2 of the *Principia,* that if homogeneous particles of matter were endued with a power of repulsion in the inverse ratio of their central distances, collectively they would form an elastic fluid, agreeing with atmospheric air in its mechanical properties. He does not infer from this demonstration that elastic fluids must necessarily consist of such particles; and his argument requires, that the repulsive power of each particle terminate, or very nearly so, in the adjacent particles. From the scholium to this proposition, Newton was evidently aware of the difficulty of conceiving how the repulsive action of such particles could terminate so abruptly as his supposition demands; but in order to show that such cases exist in nature, he finds a parallel one in magnetism."

On the above principles, the atomic weights of nearly all known bodies have been derived and tabulated. The subjoined table of the atomic weights of 54 chemical elements,

* Quoted by Dalton, *Phil. Trans.* 1826, pt. 1, p. 174.

is copied from the *Annual* of Liebig and Kopp, and represents the results of the latest and best analyses.

SYMBOLS AND EQUIVALENTS ADOPTED IN THE REPORT.

Aluminum	Al	=	13·7	Nickel	Ni	=	29·6
Antimony	Sb	=	129	Niobium	Nb		
Arsenic	As	=	75	Nitrogen	N	=	14
Barium	Ba	=	68·5	Norium	No		
Beryllium	Be	=	4·7	Osmium	Os	=	99·6
Bismuth	Bi	=	213	Oxygen	O	=	8
Boron	B	=	10·9	Palladium	Pd	=	53·8
Bromine	Br	=	80	Pelopium	Pe		
Cadmium	Cd	=	56	Phosphorus	P	=	32
Calcium	Ca	=	20	Platinum	Pt	=	98·7
Carbon	C	=	6	Potassium	K	=	39·2
Cerium	Ce	=	47	Rhodium	R	=	52·2
Chlorine	Cl	=	35·5	Ruthenium	Ru	=	52·2
Chromium	Cr	=	26·7	Selenium	Se	=	39·5
Cobalt	Co	=	29·5	Silicium	Si	=	21·3
Copper	Cu	=	31·7	Silver	Ag	=	108·1
Didymium	D			Sodium	Na	=	23
Erbium	E			Strontium	Sr	=	43·8
Fluorine	Fl	=	18·9	Sulphur	S	=	16
Gold	Au	=	197	Tantalum	Ta	=	184
Hydrogen	H	=	1	Tellurium	Te	=	64·2
Iodine	I	=	127·1	Terbium	Tb	=	
Iridium	Ir	=	99	Thorium	Th	=	59·6
Iron	Fe	=	28	Tin	Sn	=	59
Lanthanium	La			Titanium	Ti	=	25
Lead	Pb	=	103·7	Tungsten	W	=	95
Lithium	Li	=	6·5	Uranium	U	=	60
Magnesium	Mg	=	12·2	Vanadium	V	=	68·6
Manganese	Mn	=	27·6	Yttrium	Y		
Mercury	Hg	=	100	Zinc	Zn	=	32·6
Molybdenum	Mo	=	46	Zirconium	Zr	=	22·4

Certain interesting relations have been shown to subsist among these numbers. Thus it will be seen, that 23 of the elements are denoted by integral numbers, or in other words, that the atomic weights of 22 bodies are multiples of that of hydrogen, which is represented by unity. Dr. Thomson* has attributed the first suggestion of this simple relation to Mr. Dalton; and Kopp† writes, though less decisively, under the same impression. But I have failed to discover, in

* *Hist. of Chemistry*, vol. ii. p. 295.

† *Kopp*, B. ii. pp. 390, 391.

Dalton's writings, any evidence, that he either proposed this view himself, or even adopted it, when maintained by others. On the contrary, not one atomic weight, included in his earliest table, except that of hydrogen, is denoted by a whole number; and in his latest atomic table (1827) the important element carbon is estimated at 5·4; and though the other elements are represented by integral numbers, yet notes of interrogation and signs of plus or minus are affixed to several, which indicate his opinion, that they had not been rigidly determined by experiment, and might, therefore, be provisionally and approximatively only, measured by whole numbers. Further, in calculating, in 1810, from the constitution of water, the weight of the atom of oxygen, the keystone of atomic determinations, he observes: "if, however, any one chooses to adopt the common estimate of 85 to 15, then the relation of oxygen to hydrogen will be $5\frac{2}{3}$ to 1." There can, indeed, be no doubt that the merit of first suggesting and ingeniously maintaining this view is due to Dr. Prout. It was subsequently adopted by Dr. Thomson, as the basis of the experimental researches, recorded in his First Principles of Chemistry. In the words of Mr. W. Crum, "Thomson was again the first to perceive the truth and the importance of the discovery made by Dr. Prout. He immediately adopted it; and in November, 1818, he published a new table of atomic weights, embodying its principles, and taking advantage of all the improvements that had been made in analysis during the previous five years. . . . . It was not till 1840 that any chemist of note joined Thomson in the defence of Prout's doctrine. During these twenty-five years he maintained his principles and the correctness of his numbers almost single-handed; for, as in the case of Dalton, Prout had done a great proportion of his work when he announced his theory." But the eminently precise analyses of Berzelius, and especially his exact determination of the weight of the atom of lead, and the elaborate experiments of Dr. Turner, seemed, at the time, fatal to the hypothesis, that the atomic weights of all bodies are multiples, by a whole number, of that of hydrogen. The most

recent and most trustworthy analyses,* performed with every imaginable precaution for the special solution of this question, show, however, that the numbers representing the atomic weights of oxygen, carbon, nitrogen, sulphur, calcium, and iron, are most probably rigidly precise multiples of that of hydrogen. It appears also from the exact experiments of M. Maumené and M. Pelouze, that the atomic weights of chlorine, potassium, silver, sodium, barium, strontium, silicium, phosphorus, and arsenic, "approach so closely to multiples of ·5, or the half equivalent of hydrogen, that the differences may be safely considered as falling within the unavoidable errors of observation, and the multiple numbers assumed as the true numbers."† Professor Graham, from whose work I have taken the above conclusions, observes in a letter, with which he has just favoured me: "It is a view, however, which must be recognized with extreme caution, very faulty numbers having been proposed at different times under its authority, as 36 for chlorine instead of 35·5; 24 for sodium instead of 23; 40 for potassium instead of 39. The apparent tendency of discovery is no doubt favourable to the view, or at least to that modification of it, in which the half equivalent of hydrogen is made the unit." It is singularly interesting to learn from Mr. Crum, the spirit in which the veteran Thomson received the announcement of these successive confirmations of his sagacious views. "I remember having had the chance to announce to Dr. Thomson one of these substances, I forget which, as having dropped into his list. I need not tell his friends, that no expression of triumph or gratification escaped him. He took the result as a matter of course, and was confident, that for other numbers also it was only a question of time."

Other remarkable relations appear to exist between the atomic weights of certain bodies, which are characterized by similar chemical properties, and which are generally found

* Dumas and Stas for carbon, confirmed by Erdmann and Marchand; Marignac and Anderson for azote; and Dumas for hydrogen, azote, and calcium.

† Graham's *Elements of Chemistry*, vol. i. p. 111; also Art. "Atomgewichte," *Handw. Supplt.* p. 412.

associated together in nature. Thus the atomic weights of manganese and iron; of cobalt and nickel; of rhodium and ruthenium; of platinum, iridium, and osmium; are either absolutely or very nearly identical, and the atomic weight of the last triad is about half that of gold; the atom of palladium is nearly half that of silver. The atomic weights of oxygen, sulphur, selenium, tellurium, and antimony, are 8, 16, 39·5, 64·2, 129, or nearly as 1 : 2 : 5 : 8 : 16. Chlorine, bromine, and iodine are linked together by the closest chemical resemblances and are always found together in sea water and most saline springs. It is at least a remarkable coincidence, that the atomic weight of bromine, 80, is nearly a mean between those of chlorine and iodine $\left(\frac{35{\cdot}5 + 127{\cdot}1}{2} = 81{\cdot}3\right)$. In like manner the atomic weight of sodium is almost exactly the mean between those of lithium and potassium, and that of strontium the mean of those of calcium and barium. Other examples of similar import have been assembled by Gmelin. Should these relations, and especially that between the atoms of all other bodies and the atom or half atom of hydrogen, be confirmed by future unimpeachable analyses; greater probability would attach to the speculative doctrine, that there exists only one primitive matter or "πρωτη υλη" into which the sixty-two bodies, now deemed elements, may be ultimately resolved. We shall now see, that even more remarkable relations have been discovered by rendering the diameters or volumes of atoms, in place of their weights, the objects of comparison.

The founder of the atomic philosophy did not limit himself to contemplate merely the relations of atoms, by weight, he also calculated their relative magnitudes, or diameters. Thus in treating of oxygen he states, "The diameter of a particle of oxygen, in its elastic state, is to that of one of hydrogen as ·794 to 1." For the diameter of an elastic particle is as $\sqrt[3]{}$ (weight of one atom ÷ specific gravity of the fluid). Whence denoting the weight of an atom of hydrogen by 1, and the specific gravity of hydrogen also by 1, the weight of an atom of oxygen will be 7, and the specific gravity of

oxygenous gas 14; we have then $\sqrt[3]{\frac{7}{14}} : 1$ or $\sqrt[3]{\frac{1}{2}} : 1$ or ·794 : 1 : : diameter of an atom of oxygen : diameter of one of hydrogen."

As the numbers now ascertained for the weight of the atom and specific gravity of oxygen are 8 and 16, the ratio is undisturbed, and Dalton's calculated diameter is correct. He was also *near* the truth in affirming, "that the diameter of the elastic atom of chlorine is *nearly* the same as hydrogen, and may therefore be denoted by 1, but it is rather less; and the number of atoms in a given volume of this gas is to the number of atoms in the same volume of hydrogen as 106 to 100 nearly." He estimates the atomic diameter of azote as ·747, that of hydrogen being unity. It is now regarded as the same as that of hydrogen. But on Dalton's assumption that nitrous gas is the binary compound of oxygen and nitrogen, the atomic diameter of nitrogen would be the same as that of oxygen or ·794. The error is due to his estimating the atomic weight of azote 5 instead of 7 ($\frac{14}{2}$).

This investigation into the relative magnitudes of atoms has been, of late years, prosecuted with much ardour and considerable, though still limited, success, both in this country and in Germany. It has been found more convenient to render atomic volumes, rather than diameters, the objects of comparison, because the volumes vary simply as the atomic weights divided by the specific gravities; while the diameters vary as the cube roots of those quotients. The principle on which this calculation is founded will be rendered more intelligible if we refer the atomic weights to a fixed standard of weights, as grammes, and regard the specific gravities of all solid and liquid bodies as expressing the number of grammes which a cubic centimetre of the body weighs.

$$\underset{\text{grammes.}}{\text{Then sp. gr.}} : \underset{\text{c. c.}}{1} : : \underset{\text{grammes.}}{\text{At. Wt.}} : : \underset{\text{c. c.}}{\text{At. Volume.}}$$

$$\text{At. Volume} = \frac{\text{At. Wt.}}{\text{Sp. gr.}} \text{ in cubic centimètres.}$$

The atomic volume thus derived* represents obviously the

* Or it may be thus deduced :—Let sg = the specific gravity, or absolute

entire space filled by the solid atom and its surrounding atmosphere of heat. Gmelin regards this mode of comparison as incapable of affording valuable results, because "the laws of gravitation oblige us to assign the same specific gravity to the atoms of different substances, and consequently to suppose that the different weights of these atoms are due to difference of magnitude, *e. g.* that an atom of double weight must also be of double volume." It is exceedingly interesting to observe that Dalton in treating of the general properties of the metals (New System, vol. 1, p. 244,) has expressed almost the same opinion. "Though the metallic atoms, with their atmospheres of heat, are nearly the same size as the atoms of water and their atmospheres, yet it seems highly probable that the metallic atoms abstracted from their atmospheres are much larger than those of water in like circumstances. The former, I conceive, are large particles with highly condensed atmospheres; the latter are small particles with more extensive atmospheres, because of their less powerful attraction for heat. Hence it may be supposed the opacity of metals and their lustre are occasioned. A great quantity of solid matter, and a high condensation of heat are likely to obstruct the passage of light and to reflect it."

M. M. Kopp and Schröder in Germany, and M. M. Playfair and Joule in this country, have been the chief labourers in this interesting field of research.* The inquiry is still in progress, and much uncertainty at present attaches to many of the numerical results; but the relations already discovered among the numbers, representing the atomic volumes of several simple and a few compound bodies, are so striking,

weight of a given volume of the body; n = the number of atoms in that given volume; w = the atomic weight; and v = the atomic volume.

Then $n w = s g$

And $n = \frac{s g}{w}$

But $v = \frac{1}{n} = \frac{w}{s g}$

* For a clear and accurate statement of all that is known respecting atomic volumes, see Dr. Daubeny, *On the Atomic Theory*, chap. ix. p. 271.

and contribute so much fresh evidence in support of the Daltonian philosophy, that I deem it expedient to give a brief summary of the best established facts. This I have mainly gathered from a recent article by Kopp himself (Atomvolumen) in the supplement to Liebig and Poggendorff's Dictionary, p. 414, 1851.

It will be seen on inspection of the annexed table of the atomic volumes of the permanent gases and vapours, that the atomic volumes both of the simple and compound gases are either identical or stand in a simple relation to one another. If compared with sulphur vapour, the lowest term in the series, the atomic volumes of the other gases are multiples of that of sulphur by the numbers 3, 6, and 12. In organic compounds it happens most frequently, that the atomic weight of the body in the gaseous state occupies four times the space that an atom of oxygen does.

Substances.	Formula.	Atomic Weight.	Sp. gr.	Atomic Volume.
Sulphur	S	200	6·645	30·1
Oxygen	O	100	1·106	90·4
Phosphorus	P	400	4·424	90·4
Arsenic	As	939	10·39	90·4
Arsenious acid	As_2O_3	1239	13·71	90·4
Hydrogen	H	12·5	0·0691	180·8
Nitrogen	N	175	0·968	180·8
Chlorine	Cl	443·3	2·452	180·8
Bromine	Br	999	5·526	180·8
Iodine	I	1586	8·772	180·8
Mercury	Hg	1250	6·914	180·8
Water	H_2O	112·5	0·622	180·8
Sulphuretted hydrogen	H_2S	2125	1·175	180·8
Carbonic acid	CO_2	275	1·521	180·8
Nitrous oxide	N_2O	275	1·521	180·8
Nitric oxide	NO_2	375	1·037	361·6
Muriatic acid	HCl	455·8	1·260	361·6
Ammonia	NH_3	212·5	0·588	361·6
Chloride of ethyl	C_4H_5Cl	805·8	2·228	361·6
Valerianate of oxide of ethyl	$C_{14}H_{14}O_4$	1625	4·494	361·6

Great, and hitherto insurmountable, difficulties present themselves to the rigorous determination of the atomic volumes of solids and liquids, especially of the former. For the divisor in the formula, $\frac{\text{at. wt.}}{\text{sp. gr.}}$, is not a constant quan-

tity, nor does it vary equably, according to the same law, in solids and liquids, as it does in the gases. Thus it has been shown by Playfair and Joule, that the specific gravity of solids varies largely in the same body with its state of aggregation; that of platinum between the wide limits of 17·766 and 21·206. Moreover the specific gravity of all solids and liquids varies in each body with its temperature, and according to a different ratio in each different body. It has been sought to escape from this difficulty by comparing the atomic volumes of solids and liquids, not at the same, but at what have been termed *corresponding* temperatures. These, if ascertainable, would be such at which equal increments of heat, or more strictly, of temperature, produce equal expansions. As respects liquids, it has been conjectured, and is not improbable, that those are *corresponding* temperatures, at which the forces of their vapours are equal, viz., their respective boiling points; and according to Dalton's early discovery, temperatures, equidistant from those boiling points, would correspond approximately. But as regards matter in the solid condition, there exists no principle, from which correspondence of temperature can be derived, unless it be assumed to exist at their respective melting points, as is conjectured by Kopp.* This strikes me, however, as eminently improbable, as being inconsistent with the known unequal expansions of liquids, under which category melted solids must be included. The numbers, therefore, obtained by MM. Kopp and Schröder, can only be regarded as approximations, more or less remote, to the true atomic volumes of solids and liquids; nor does Dr. Kopp himself claim higher authority for them. As such, however, they indicate the existence of certain simple relations, among various groups of bodies, characterized by the possession of similar chemical properties, and generally found associated together in the natural kingdom. In order to render these relations more conspicuous, I have altered the arrangement of the solids and liquids, in the subjoined table from Dr. Kopp; and have

* *Atom volumen*, supplt. p. 418.

omitted the atomic weights and specific gravities, of which the atomic volumes are the quotients. It is manifest that there exists as close an approximation to identity of atomic volume in the allied chemical *elements*, here classed in groups, as is consistent with the limits of experimental error in so difficult an investigation. The atomic volume of gold is also exactly half that of silver and of tellurium.

	At. Vol.
Antimony	240
Arsenic	167
Lead	114
Cadmium	80
Potassium	569
Sodium	297
Phosphorus	204
Mercury	92
Bismuth	136
Tin	95
Zinc	59

	At. Vol.
Bromine	313
Chlorine	333
Iodine	320
Sulphur	101
Selenium	102
Chromium	47
Iron	45
Cobalt	43
Copper	44
Manganese	43
Nickel	42
Molybdenum	69
Wolfram	69
Iridium	57
Palladium	56
Platinum	57
Rhodium	58
Gold	64
Silver	128
Tellurium	128

Identity of atomic volume has also been established for such *compound* bodies as are isomorphous.* Thus carbonate of strontia and carbonate of lead, bodies of similar atomic composition, have almost precisely the same crystalline form; and their atomic volumes are 256 and 258, numbers nearly identical. Yet agreement in atomic volume is not to be regarded as the cause of agreement in crystalline form, for though identity of form in bodies of analogous composition

* Kopp, *Atom volumen*, p. 418.

involves identity of atomic volume, the converse is not true, identity of atomic volume by no means involving identity of crystalline form. A dimorphous body possesses in each of its modifications a different specific gravity, and consequently a different atomic volume.

It has not as yet been clearly ascertained, in what relations the atomic volume of a solid or liquid compound stands to the atomic volumes of its constituents; or what are to be assumed as the atomic volumes of those constituents in combination. As respects the oxides and other compounds of the *heavy* metals, if it be assumed that those metals possess the same atomic volumes in combination as in their isolated state, numbers are derived for the atomic volume of oxygen and certain complex acid radicals, from the oxides or salts of *different* metals, that approach nearly to identity. Thus the atomic volumes of the oxides of copper and zinc are 76 and 91; and if from these numbers be subtracted the atomic volumes of the metals in the above table, 44 and 59, the same number, 32, remains for the atomic volume of oxygen. The atomic volume of protoxide of lead is also $= 114+32 = 146$; and that of the peroxide of iron, $Fe_2 O_3$, is equal to the atomic volume of the metal $+ 96$ (3×32). On the same principle, if from the atomic volumes of nitrate of lead 472, and nitrate of silver 486, be deducted the atomic volumes of those metals, 114 and 128, we obtain in both alike the number 358, as representing the atomic volume of the compound radical $\ddot{N}O_6$. The atomic volumes of all the above compounds seem, therefore, to be equal to the sum of the atomic volumes of their constituents. But this simple relation is so far from obtaining in the combinations of the metallic bases of the alkalies and earths, that the atomic volume of an atom of sulphate of potash, 420, is inferior to that of one of its constituents, potassium, 569.

In a series of complex organic products,* in the state of liquid, which differ from one another, by $C_2\ddot{H}_2$ or its multiples, numerical relations among their atomic volumes have been observed, which indicate that $C_2\ddot{H}_2$ has an atomic volume

* *Atom volumen*, p. 422.

equal to 248, or some number approaching that, and increases, in entering into combination, the atomic volume of the compound, to that amount. Thus:

Wood-spirit	$C_2H_4O_2$	$=529$
Alcohol	$C_2H_4O_2 + C_2H_2$	$=777 = 529 + 248$
Hydrate of oxide of amyl	$C_2H_4O_2 + 4(C_2H_2)$	$=1541 = 529 + 4 \times 253$

Again

The atomic volume of ether	$C_4H_5O = 664$
That of water	$HO = 117$
The sum of these formulæ gives that of alcohol	$C_4H_6O_2 = 781$ a number

differing very slightly from 777, obtained experimentally, as the atomic volume of alcohol. The experimental numbers for all these liquids were observed at their respective boiling points. Dr. Kopp concludes that, in respect to the atomic volumes, both of liquid and solid compounds, the existence of relations is at present only indicated; and that the laws, lying at the base of those relations, are yet but imperfectly investigated.

The simple fact, that some of our ablest practical workers and thinkers are actually engaged in the attempt to determine the relative *magnitudes* of *atoms*, furnishes in itself no mean evidence of their confidence in the philosophy of Dalton. In the present early stage of the inquiry, it would be premature to place implicit reliance in the numbers that have been already tabulated. Yet, viewed as approximative and provisional results merely, they supply several remarkable analogies. Thus chlorine, iodine, and bromine, electronegative elements long classed together, as possessing the most marked chemical resemblances, and invariably found associated in saline sources, as if simultaneously elaborated by the grand chemical operations of nature, are now shown to have atomic volumes, approaching nearly to identity. The same relation obtains in the atomic volumes of iridium, palladium, platinum, and rhodium, all noble metals, allied by many common properties, and never found apart in nature; and in those of chromium, iron, manganese, cobalt, nickel, and copper, (classed together as oxidizable metals proper, whose oxides form powerful bases,) giving birth to isomorphous salts, and, it is

worthy of remark, found associated in those mysterious meteoric masses, from which we obtain our only glimpses of the chemistry of bodies, revolving in the celestial spaces, and infer their identity of composition with our earth. Of greater significance in their bearing upon the atomic hypothesis, are the facts, respecting the oxides and salts of certain of the heavier metals, from which it is a highly probable inference that the atomic volume of the compound is equal to the sum of the atomic volumes of its components. This relation is fatal to the doctrine of interpenetration of elements, and points to the apposition, side by side, of atoms of definite magnitude, occupying, when thus propejacent and combined, the same space as when isolated.

On referring to the chapter on Chemical Synthesis above, it will be found to conclude with an allusion to plates, "containing the arbitrary marks or signs, chosen to represent the several chemical elements or ultimate particles." One such plate accompanied the first part, and two others the second part of volume I., New System. On that marked 5 may be seen represented four supposed compounds of hydrogen and oxygen, *viz.* water, ⊙○; fluoric acid, ○⊙○; muriatic acid, ; and oxymuriatic acid, ; in which one atom of hydrogen ⊙ is conceived to combine respectively with 1, 2, 3, and 4 atoms of oxygen ○. This view, Dalton of course afterwards abandoned; and he introduced, from time to time, with the progress of science, changes in the notation of other compound bodies. But he continued, during the whole of his life, to employ the same system of atomic symbols, and caused the annexed plate to be struck off, to illustrate a "Lecture on the atomic System of Chemistry," which he delivered October 19, 1835, to the members of the Manchester Mechanics' Institution. He steadily persisted in denying the superior precision and expressiveness of the admirable system of chemical formulæ, proposed by Berzelius in 1815,* and now employed by all European chemists.

* I observe that the first suggestion of alphabetical symbols, employed in the construction of formulæ, is claimed by Dr. R. D. Thomson for his late distinguished relative, and is attributed to Dr. Thomson by Berzelius himself, in a

In a letter addressed to Professor Graham, April 1837, not long before his paralytic seizure, he thus strongly condemns the Berzelian notation:

"Berzelius's symbols are horrifying; a young student in chemistry might as soon learn Hebrew as make himself acquainted with them. They appear like a chaos of atoms. Why not put them together in some sort of order? Is not the *allocation* a subject of investigation as well as the weight? If one order is found more consistent than another, why not adopt it till a better is found? Nothing has surprised me more than that such a system of symbols should ever have obtained a footing anywhere." Again in another document, he says, "I do not, however, approve of his adopting and defending the chemical symbols of Berzelius, which appear to me equally to perplex the adepts of science, to discourage the learner, as well as to cloud the beauty and simplicity of the atomic theory."

Dalton was not satisfied with merely pictorial representations of atoms. In his latest memoir; "On the analysis of sugar," he has recorded, "My friend, Mr. Ewart, at my suggestion, made me a number of equal balls about an inch in diameter about thirty years ago; they have been in use ever since, I occasionally showing them to my pupils. One ball had 12 holes in it equidistant, and twelve pins were stuck in the other balls so as to arrange the 12 around the one and be in contact with it; they (the 12) were about $\frac{1}{10}$th of an inch asunder. Another ball, with 8 equidistant holes in it; and they (the 8) were about $\frac{3}{10}$ of an inch asunder, a regular series of equidistant atoms. I had no idea at the time that the atoms were all of a bulk, but for the sake of illustration I had them made alike." Dr. George Wilson informs me, "I could find no traces of them two years ago in Manchester. They are objects of historical interest, and should, if existing, be preserved."

During the latter portion of Dalton's active career, and

work published in 1814, where he says, that he "strictly followed the rules for this purpose given by Thomson in his *System of Chemistry*." — Mr. Crum's *Memoir*, p. 264.

especially since its close, in 1837, on his first formidable illness, there have been no essential changes, involving first principles, in that department of the science which he exclusively cultivated,—inorganic chemistry. At the present time, and for the last quarter of a century, the efforts of our ablest experimenters and most original thinkers have been concentrated upon the chemistry of organic substances; and a new science has been called into existence—vast, in domains already explored, and boundless in prospects of future research. The atomic hypothesis is therefore brought into the presence of a large assemblage of facts, unknown and even unconjectured by its author; and its power to interpret and embrace this fresh order of phenomena within its lines of generalization would, if established, furnish no inconsiderable argument in favour of its soundness and comprehensiveness.

1. It is a significant fact that all labourers in this field of research concur in employing the language of the atomic theory. Mere *empirical* formulæ, or such as represent the direct results of analysis, are wholly superseded by *rational* formulæ, or such as state the inferred number of simple atoms which make up one compound atom, and even the proximate groups which they are supposed to form. All organic bodies are now defined and distinguished by such atomic formulæ; and it is impossible to conceive the very existence of so vast a body of exact knowledge, comprehending compounds of such extreme complexity, except as recorded in this clear, precise, and universal language. The general prevalence then of rational or atomic formulæ in this new and rapidly progressive branch of chemistry is in itself a tacit but signal homage to the theory of Dalton.

2. The law of substitution, which has been shown by the researches of Regnault, Dumas, and others to preside over the formation of large classes of organic products, is in its very essence atomic. Thus in these interesting series of changes, it has been found that the hydrogen atoms of the organic body yield their places successively, one by one, to an equal number of atoms of chlorine, iodine, bromine, and

a few acid radicals; giving birth to a series of new products, departing at each successive step further from the primitive type, which is still however not wholly destroyed. The best examples of substitution products are the following of the chloride of ethyl series and the parallel terms of the Dutch liquid series, which I have derived from Dr. Hofmann's Fifteenth Lecture.

Chloride of ethyl	C^4H^5Cl		
„ monochlorinetted	$C^4(H^4Cl)Cl$	Dutch liquid	$C^4(H^3Cl)HCl$
„ dichlorinetted.....	$C^4(H^3Cl^2)Cl$	Chlorinetted do	$C^4(H^2Cl^2)HCl$
„ trichlorinetted	$C^4(H^2Cl^3)Cl$	Dichlorinetted do.....	$C^4(HCl^3)HCl$
„ quadrichlorinetted	$C^4(HCl^4)Cl$	Trichlorinetted do.	C^4Cl^4HCl
„ quintichlorinetted	C^4Cl^5Cl	Quadrichlorinetted do. or sesquichloride of carbon	C^4Cl^6

In the five terms of each of the above series, we observe successively the elimination of a single atom of hydrogen and its replacement by a single atom of chlorine, till in the last terms of both series all the atoms of hydrogen have disappeared and their places are filled by the same number of atoms of chlorine. It is also important to remark that the several terms of the two series, standing on the same line, are identical in *ultimate* composition. The final products of the action of chlorine upon chloride of ethyl and Dutch liquid seem to be also identical in their properties; but the intermediate products of the two series, which are identical in composition, differ from each other in their boiling points and other characteristic properties. They are therefore isomeric, or more strictly metameric compounds; and point to a different *grouping of the same atoms,* as the only cause of diversity in their chemical properties.

Instances of similar atomic substitutions, in which oxygen atoms are replaced by atoms of sulphur and even by compound molecules, as the radicals of sulphuric and oxalic acids are furnished by the interesting derivatives of alcohol. Thus, as will be more distinctly seen on the annexed diagrams, copied from Dr. Hofmann, alcohol, $C_4H_6O_2$, is transformed into mercaptan, $C_4H_6S_2$, by the substitution of two atoms of sulphur for its two atoms of oxygen; and into

sulphovinic and oxalovinic acids by the replacement of the same two atoms of oxygen by two compound molecules, consisting respectively of one atom sulphur and four atoms oxygen, and of two atoms carbon and four oxygen. Alcohol is also converted into potassium alcohol by the exchange of one atom of hydrogen for one of potassium.

3. On the extensive class of substitution phenomena has been founded by the French chemists, M. Laurent and

M. Dumas, the theory of chemical types. In the special examples of replacement first enumerated, it was shown that the substitution of atoms of chlorine, an electro-negative element, for those of hydrogen, an electro-positive element, was attended by no essential change in the fundamental properties of the compound. Hence it has been inferred that the chemical properties of such complex substances depend on the number and mode of grouping of their constituent atoms, rather than on the nature of those atoms; and the primitive group, or compound atom, from which the derivatives are formed by replacement, is called a chemical type. Naphthaline furnishes a good illustration of a complex molecular type, out of which an almost infinite variety of derivatives may be obtained, by replacing its eight hydrogen atoms with chlorine, iodine, bromine, or nitric acid. The number of permutations is further augmemted by the existence of isomeric compounds, in which the same elements are combined in exactly the same proportions. Thus, Laurent describes seven cases of chlo-naphthese, $C^{20}H^{6}Cl^{2}$; and accounts for their existence on the principle that differences in properties may depend on the relative position in the compound group of the atoms of hydrogen displaced. Thus the dislodgment of the atoms of hydrogen No. 1 and No. 2

Naphthaline. C20. H8.

by chlorine may gave birth to one compound, and that of No. 3 and No. 4 to another distinct derivative. These pictorial representations of molecular types, as Dr. Daubeny, from whose work I have copied that subjoined, observes, bring us back to the Daltonian symbols, and are essentially and emphatically atomic.

4. Not less significant in its bearing on the atomic doctrine is the law of Homologues, first pointed out by M. Gerhardt. On comparing the atomic formulæ of large classes of analogous organic substances, as the series of hydrocarbons, radicals, ethers, alcohols, aldehydes, and organic acids, they have been discovered to be remarkably symmetrical. The homologous alcohols and acids are arranged as below in series, in which it will be seen that the oxygen constituent is a constant quantity, while the carbon and hydrogen atoms are increased in each successive term by the addition either of C_2H_2 or a multiple of it. The number of atoms of carbon in the corresponding alcohols and acids is the same; the hydrogen atoms in the acids are two less than in the parallel alcohols, but are replaced by two atoms of oxygen; so that each compound acid atom contains the same number of simple atoms as its parallel alcohol. It will be observed that the acid series is far more complete than the alcohol series. The intermediate terms left blank in the alcohols doubtless exist, and must be speedily discovered, in Dr. Hofmann's emphatic words, "perhaps to-morrow."

Methyl alcohol	$C^2H^4O^2$	.....	Formic acid	$C^2H^2O^4$
Ethyl alcohol	$C^4H^6O^2$	.....	Acetic acid	$C^4H^4O^4$
„	$C^6H^8O^2$	.....	Propionic acid	$C^6H^6O^4$
„	$C^8H^{10}O^2$	.....	Butyric acid	$C^8H^8O^4$
Amyl alcohol	$C^{10}H^{12}O^2$	.....	Valeric acid	$C^{10}H^{10}O^4$
„	$C^{12}H^{14}O^2$	.....	Caproic acid	$C^{12}H^{12}O^4$
„	$C^{14}H^{16}O^2$	.....	Œnanthylic acid	$C^{14}H^{14}O^4$
Capryl alcohol	$C^{16}H^{18}O^2$	.....	Caprylic acid	$C^{16}H^{16}O^4$

Finally, we may safely accept Dr. Hofmann's masterly lectures, delivered last spring in the laboratory of the Royal Institution, as faithfully representing the actual condition of a science which he has enriched with such important contributions. Now these lectures were *emphatically atomic* in

the entire scope and spirit of their teaching; in his own words, from a letter to me "they were conceived, to a certain extent, in Dalton's spirit." Thus, in his lecture on alcohol and its derivatives, he stated, "Chemists have not been satisfied with the elaboration of the quantitative composition of these bodies, and of the numerical relation in which they stand to each other; they have gone a step farther, and endeavoured to throw some light upon the mode in which the elements are arranged in those substances, in one word *upon their molecular* constitution . . . . By the united efforts of a considerable number of inquirers, these substances have, as it were, become *transparent to the mental eye* which traces their *internal construction,* and thus affords a perception of relations, which but a few years ago appeared utterly inaccessible to observation." All Dr. Hofmann's lectures were illustrated by elaborate *atomic* symbols and diagrams; and the most complex changes and replacements were brought distinctly before the eye by an ingenious sliding frame, in which coloured atomic signs were placed in juxtaposition or eliminated, with extreme facility and speed. Dalton was ever wont to affirm, that no conception was clearly grasped by the intellect, if it could not be visibly depicted or embodied to the external sense;—and I can conceive of no objects which he would have contemplated with such complacent approval as this skilful mechanical contrivance and these coloured portraitures of his favourite atoms.

Before terminating this imperfect narrative of the develevelopment of the atomic doctrine in the mind of its great author, and of the confirmation it successively received from the labours of European chemists, it may be expedient to assemble here, in short compass, the judgments pronounced upon it recently and at the present time by the highest authorities. In the estimation of Berzelius, 1835, "the atomic hypothesis was afterwards confirmed by numerous experiments, and we may state without exaggeration that this is one of the greatest steps which chemistry has yet made towards perfection." Mitscherlich thinks,* "that this

* *Lehrbuch der Chemie,* B. 1, p. 434.

hypothesis, like every other, must undergo changes, in proportion as observations are multiplied. It is possible, although highly improbable, that it may be wholly superseded by another; yet the history of science can adduce scarcely any law, and certainly no theory, which has conducted the inquirer to so many discoveries as this hypothesis; it has served as a guide not merely in the discovery of the law of definite proportions, but also in many investigations which have given the most important results." Mitscherlich has expressed even more strongly his adhesion to atomic views, in his memoir on benzin and its compounds, 1834. He here describes a class of organic substances formed by the combination of two compound bodies or of a simple and a compound body, out of which combination a part has eliminated itself. Thus 4 atoms benzin, containing together 12 atoms carbon and 12 atoms hydrogen, combine with oxygen compounds, so that an atom of water separates itself from the combination. "The admission of this class of atoms is derived readily from the atomistic theory, since such a combination and its elimination can take place where *the atoms* of the two combined substances *lie nearest one another*. The fact itself furnishes a clearer view than could have been hitherto framed of the *juxtaposition* (nebeneinanderliegen) *of atoms*."—*Über das Benzin*, p. 3. Professor Graham states:—"But the first foundations of a complete system of equivalents, embracing both simple bodies and their compounds, were laid by Dalton at the same time that he announced his atomic theory." Again:—"The laws of combination and the doctrine of equivalents which have just been considered, are founded upon experimental evidence only, and involve no hypothesis. The most general of these laws were not, however, suggested by observation, but by a theory of the atomic constitution of bodies in which they are included, and which affords a luminous explanation of them. The partial verification which this theory has received in the establishment of these laws adds greatly to its interest, and is a strong argument in favour of its truth." Gmelin conceives that "the origin of these two laws" of multiple and

reciprocal proportions "is most satisfactorily explained by the atomic theory, according to which every simple substance consists of very small invisible particles, called atoms; these atoms being of uniform weight and volume in each individual substance, while the atoms of different substances may be of different weight and volume." Finally, in the judgment of Sir John Herschel:—"The atomic theory, or the law of definite proportions, which is the same thing presented in a form divested of all hypothesis, after the laws of mechanics is, perhaps, the *most important* which *the study of nature has yet disclosed.* The extreme simplicity which characterizes it, and which is itself an indication, not unequivocal, of its elevated rank in the scale of physical truths, had the effect of causing it to be announced at once by Mr. Dalton, in its most general terms, on the contemplation of a few instances, without passing through subordinate stages of painful inductive ascent by the intermedium of subordinate laws; such as, had the contrary course been pursued by him, would have been naturally preparatory to it, and such as would have led others to it by the prosecution of Wenzel's and Richter's researches, had they been duly attended to." — *Prelim. Discourse,* p. 305.

I cannot more worthily terminate this important chapter than with the following extracts from letters, with which I have been lately honoured by Dr. Faraday and Baron Liebig, in reply to inquiries respecting their present atomic opinions.

"Royal Institution, August 2, 1853.

"I do not know that I am unorthodox, as respects the atomic hypothesis. *I believe in matter and its atoms* as freely as most people, at least I think so. As to the little solid particles, which are by some supposed to exist independent of the forces of matter, and which in different substances are imagined to have different amounts of these forces associated with, or conferred upon them, (and which even in the same substance, when in the solid, liquid, and gaseous state, are supposed to have like different proportions of these powers) as I cannot form any idea of them apart from the forces, so

I neither admit nor deny them. They do not afford me the least help in my endeavour to form an idea of a particle of matter. On the contrary they greatly embarrass me; for after taking account of all the properties of matter, and allowing in my consideration for them, then these nuclei remain on the mind, and I cannot tell what to do with them. The notion of a solid nucleus without properties is a natural figure or stepping-stone to the mind at its first entrance on the consideration of natural phenomena; but when it has become instructed, the like notion of a solid nucleus, apart from the repulsion, which gives our only notion of solidity, or the gravity, which gives our notion of weight, is to me too difficult for comprehension; and so the notion becomes to me hypothetical, and what is more, a very clumsy hypothesis. At that point, then, I reserve my mind as I feel bound to do in hundreds of other cases in natural knowledge."

Dr. Faraday's conception of the ultimate constitution of matter closely agrees with that of Boscovich, and with that, claimed for Dr. Hutton by his learned biographer, Professor Playfair. "The most accurate examination of the properties of body confirms the truth of the opinion that it is composed of unextended elements. . . . . Thus the supposed impenetrability, and of course the extension of body, is nothing else than the effort of a resisting or repulsive power; its cohesion, weight, &c., the efforts of attractive power; and so with respect to all its other properties. But if this be granted, and if it be true that in the material world every phenomenon can be explained by the existence of power, the supposition of extended particles as a *substratum* or residence for such power, is a mere hypothesis, without any countenance from the matter of fact . . . . Such particles ought to be entirely discarded from any theory that proposes to explain the phenomena of the natural world."—*Playfair's Works*, vol. iv, p. 84.

Letter from Baron Liebig.

"Munich, September 20, 1853.

"You wish to have from me, my view of the hypothesis of Dalton, and of its influence on the development of chemistry. This is a difficult problem; for we, who stand in presence of the science as now constituted, can scarcely conceive how it would have developed itself without this hypothesis. All our ideas are so interwoven with the Daltonian theory, that we cannot transpose ourselves into the time, when it did not exist. Dalton's atomic theory was a product of the age, and sprang forth in his mind, as a consequence of the discovery of chemical proportions or equivalents. By means of this theory, the numerical results were connected with definite representations concerning the internal nature and constitution of chemical combinations; and thus a number of researches were called forth and prepared for, which had for their object the relations of physical properties to internal composition. I need refer only to isomorphism, to the relations of the specific heat to the atomic weight, and to those of the boiling point to composition. Chemistry received in the atomic theory, a fundamental view; which overruled and governed all other theoretical views, to which the ideas of the age respecting chemical forces, affinity, cohesion referred themselves; it was the bond which bound together all other views. In this lies the extraordinary service which this theory rendered to science, viz.: that it supplied a fertile soil for further advancement; a soil which was previously wanting. In the most recent investigations concerning the constitution of organic bases, the alcohols and the acids corresponding to the alcohols, we have seen that the groundwork of the Daltonian theory is equally valid for organic bodies. His main law that the properties of compounds are dependent on the nature of their elements, and on the mode and way of their position or arrangement, will always maintain a high value."

CHAPTER VI.

PUBLICATION OF PART II. OF VOLUME I. "NEW SYSTEM" — POTASSIUM AND SODIUM AND CHLORINE CONTROVERSIES — LETTER FROM SIR H. DAVY — LAW OF VOLUMES — CHARCOAL—ESSAYS ON THE PHOSPHATES, SALTS OF AMMONIA, SULPHURIC ETHER, OIL GAS, AND METEOROLOGY—MR. OTLEY'S ACCOUNT OF THEIR JOINT MOUNTAIN EXCURSIONS IN THE LAKE DISTRICT.

In November, 1810, appeared the second part of the first volume of a *New System of Chemical Philosophy*. It was dedicated to Sir H. Davy (then Mr. Davy and Sec. R. S.) and to my father, "as a testimony to their distinguished merit in the promotion of chemical science and as an acknowledgment of their friendly communications and assistance." To his brother he writes, November 17, "Herewith I send six copies of my *Chemistry*, Part II, which I have just brought out: the work is not yet finished; but I have no doubt the judicious readers will thank me for the delay, having been spending a great part of my time for the two last years in prosecuting inquiries the results of which are now published."

I extract from the preface the following interesting passages:—

"When the first part of this work was published I expected to complete it in little more than a year; now two years and a half have elapsed, and it is yet in a state of imperfection. The reason of it is, the great range of experiments which I have found necessary to take. Having been in my progress so often misled by taking for granted the results of others, I have determined to write as little as possible but what I can attest by own experience. On this account, the following work will be found to contain more

original facts and experiments than any other of its size on the elementary principles of chemistry. I do not mean to say that I have copied the minutes of my note-book: this would be almost as reprehensible as writing without any experience. Those who are conversant in practical chemistry know that not more than one new experiment in five is fit to be reported to the public; the rest are found upon due reflection to be some way or other defective, and are useful only as they show the sources of error and the means of avoiding it. . . . . .

"Whatever may be the result of my plan to render the work somewhat like complete by the addition of another volume, I feel great satisfaction in having been enabled thus far to develope *that theory of chemical synthesis, which the longer I contemplate the more I am convinced of its truth.* Enough is already done to enable any one to form a judgment of it. The facts and observations yet in reserve are only of the same kind as those already advanced; if the latter are not sufficient to convince, the addition of the former will be but of little avail. In the meantime, those who with me adopt the system will, I have no doubt, find it a very useful guide in the prosecution of all chemical investigations."

It would be manifestly inexpedient now, after the lapse of more than forty years, to review minutely a work devoted to the details of so rapidly progressive a science as experimental chemistry. This second part, written and printed in sections at various intervals, during a period of nearly three years, furnishes however a curious revelation, both of the fluctuations of opinion in its author's mind, and of the chaotic state of the science itself, out of which the genius of Davy was labouring to educe order and symmetry. Thus in the early portion of the book, Dalton classes potassium and sodium among the metallic elements, and, though not finally and irrevocably adopting Davy's views (for he limits his adhesion by the words, "This subject must be left to future experience") he undoubtedly prefers them to the doctrine of Gay-Lussac and Thénard, that potassium is a compound of

hydrogen and potash. But in the chapter on the alkalies, written probably after the lapse of nearly two years, he asserts it to be "most probable that these metals are compounds of potash and soda with hydrogen, and that the two fixed alkalies still remain among the undecompounded bodies." He was even more inflexible in refusing his assent to the new chlorine theory. He had adopted the singular notion that, water consisting of an atom of oxygen united with one of hydrogen, fluoric acid consisted of one hydrogen with two of oxygen, muriatic acid of one hydrogen with three of oxygen, and oxymuriatic acid of one hydrogen with four oxygen. Even in the Appendix, which was probably written about the period of publication, he thus concludes: "Mr. Davy has given several experiments to show that oxymuriatic acid combines with hydrogen to produce muriatic acid, but none of them appears to me decisive." Davy discusses both these questions, the constitution of the alkalies and that of muriatic acid, in the following important letter of June 20, 1810.

"My dear Sir,—Before you decide upon the questions discussed in your last letter I wish you would try two or three experiments.

"In my first experiments upon potassium, when I used a very feeble power upon fused potash, I saw no effervescence, but whenever I try the experiment now with a battery ten times as strong there is a copious disengagement of hydrogen. It is not *fusible* potash which is formed by the combustion of potassium, but a potash infusible at a dull red heat. Besides if you moisten any metallic oxide and decompose it by electricity little or no hydrogen is disengaged. Would you say in this case that the hydrogen exists in the metal?

"If you will mix in an exhausted vessel one part of oxymuriatic acid and 1·1 part of hydrogen, you will find that they will make nearly 2 of muriatic acid gas: there is no more moisture apparent than would result from the vapour in the gases, which precipitates a little muriatic acid gas.

"There is a more simple experiment which any one may make. Keep muriatic acid gas over dry mercury for some

days, calomel will be formed, and .5 of hydrogen for every 1 of muriatic acid gas absorbed will be formed. If mercury be ignited in the vapour by voltaic electricity in muriatic acid gas, it is instantly decomposed, calomel formed, and half its volume of hydrogen evolved. That muriatic acid gas is a compound of hydrogen and oxymuriatic acid, I conceive I am able to prove in as distinct a manner as that carbonic acid is composed of oxygen and charcoal. You may depend upon the correctness of Gay-Lussac and Thénard's experiments on the gaseous oxide of carbon. They are not always accurate, but I have twice had the same result. If you use a dry exhausted vessel to receive the gases in, depend upon it your *sunning* will not succeed.* I am much obliged to you for your account of your mode of making heavy phosphuretted hydrogen. I have not yet been able to make any experiments on that subject.

"I am, my dear Sir,

"Very sincerely yours,

"H. DAVY."

Dalton seems to have been the first person to detect the influence of solar light in causing what he terms the "rapid combustion of hydrogen in oxymuriatic acid;" or if it had been previously observed he was not aware of the fact. After observing this action of the two gases, "on repeating the experiment, a few hours afterwards, no such diminution was observed. I recollected that the sun had shone upon the instrument in the former one; it was again placed in the direct rays of the sun, and the diminution was rapid as before."

The main object of this second part was to determine, by the rules laid down in the chapter on Synthesis, the atomic weights of elementary and compound bodies. He derived

* Davy alludes to Dalton's statement, that a mixture of oxymuriatic acid gas and carbonic oxide when exposed to direct solar light is decomposed: "carbonic acid, water and muriatic acid are the results."—*New System*, vol. i. p. 301, and Appendix, p. 553. Shortly after this letter was written, Dr. Davy, in his brother's absence, repeated the experiment, and discovered a compound of carbonic oxide and chlorine, the phosgene or chloro-carbonic acid gas.

the combining proportions partly from his own experiments, but principally from a critical examination of the most trustworthy researches of others. Thus he calculates the relation of oxygen to hydrogen in water to be 7 : 1, from "the accurate experiments of Cavendish and Lavoisier, which have shown that oxygen is nearly fourteen times the weight of hydrogen," and from the fact "that in Volta's eudiometer *two* measures of hydrogen require just *one* of oxygen to saturate them." He does not state whether it was on the authority of his own experiments, or of those of Gay-Lussac and Humboldt, that he admitted this fact,—the corner-stone of the theory of volumes, which yet we shall find he afterwards strenuously repudiated. Thus, when he reaches the chapter of oxymuriatic acid, he states, "From the mean of five experiments executed as above, I am induced to conclude that 100 measures of hydrogen require 94 of oxymuriatic acid gas to convert them into *water*." Again he states that "10 measures of carbonic oxide will be found to take from 4·5 to 5 measures of oxygen," for perfect combustion; and "his conclusion is, that carbonic oxide in its combustion requires just as much oxygen as it previously has in its constitution, in order to be converted into carbonic acid." He also admits, p. 382, that "a given volume of carbonic *acid* gas contains the same number of atoms as the same volume of hydrogen;" and yet persists in maintaining that 106 measures of carbonic *oxide* contain as many atoms as 100 measures of hydrogen." His own experiment, in which 100 measures carbonic oxide took 47 oxygen, and produced 94 carbonic acid (p. 375), rightly interpreted, demonstrates the truth of Gay-Lussac's law in this special case of the compounds of oxygen and carbon. His gas doubtless contained 6 per cent. impurity; and 94 parts carbonic oxide took 47 oxygen, and produced 94 carbonic acid,—numbers rigidly conforming to the law of Gay-Lussac. It is quite inexplicable that a philosopher, whose characteristic tendency was to contemplate all empirical phenomena from a mathematical point of view, and who, in determining the relations of the gases to water, was

willing to cancel large differences between the observed and calculated numbers, in order to bring them within the compass of a simple relation of atomic distances, should reject the experimental law of Gay Lussac, establishing the existence of a simple relation in the number of atoms in a given volume of the different gases. This resistance to the strongest evidence, especially manifested in the Appendix, pp. 555 and 559,* is the more remarkable, when we find Dalton in the earlier printed chapters of this same volume admitting such relation in the important bodies, oxygen, hydrogen, and carbonic acid.

His view of the compounds of oxygen and nitrogen differs from that now prevalent, in regarding nitrous gas (nitric oxide) and not nitrous oxide as the binary compound; and consequently in halving the atomic weight of nitrogen, now received. I find from my notes, February, 1824, that he at that date also continued of the same opinion. The arguments he then advanced for supposing nitrous gas to be the binary compound were, "besides its having the least specific gravity of any of them:"—first, that by the continued electrization of atmospheric air, nitrous acid is uniformly produced, arising from the generation of nitrous gas, which immediately unites with free oxygen. Now, he regarded it as much more probable, that electrization would induce the union of an atom of azote with *one* atom of oxygen than with *two*. Second. If nitrous oxide be electrified in a similar manner, we also obtain nitrous acid. He contended that by the first electrization an atom of azote is struck off from the ternary atom of nitrous oxide ⊕○⊕ leaving an atom of nitrous gas. The effect of a second electrization will probably be to detach two atoms of azote from some of the atoms of nitrous oxide, and the atoms of oxygen thus liberated will combine with the previously formed nitrous gas. Whereas, if nitrous

* "The truth is, I believe, that gases do not unite in equal or exact measures in any one instance; when they appear to do so, it is owing to the inaccuracy of our experiments. In no case, perhaps, is there a nearer approach to mathematical exactness, than in that of 1 measure of oxygen to 2 of hydrogen; but here the most exact experiments I have ever made gave 1·97 hydrogen to 1 oxygen."

oxide be the binary compound, free oxygen and free nitrogen should be the only products of its electrization. This argument does not, I confess, strike me as so forcible as the first. In his letter to Dr. Daubeny,* he still maintains the same views.

The two combinations of hydrogen and charcoal described by him, viz. carburetted hydrogen and olefiant gas, deserve notice as having been among the earliest examples that occurred to his mind of the law of multiple proportions.† Of the constitution of carburetted hydrogen, he observes:— "no correct notion seems to have been formed till the atomic theory was introduced and applied in the investigation. It was in the summer of 1804 that I collected, at various times and in various places, the inflammable gas obtained from ponds. . . . . After due examination, I was convinced, that just one half of the oxygen expended in its combustion, in Volta's eudiometer, was applied to the hydrogen, and the other half to the charcoal. This leading fact afforded a clue to its constitution." "If 100 measures of carburetted hydrogen be fired over mercury with 200 of oxygen, the result will be a diminution of near 200 measures, and the residuary 100 measures will be found to be carbonic acid." He concludes that carburetted hydrogen is a compound of one atom of charcoal and two of hydrogen; the compound atom occupies the same space (nearly) as an atom of hydrogen. From similar experiments on olefiant gas, he concluded, that an atom consists of 1 of charcoal and 1 of hydrogen. But he afterwards considered 2 of charcoal with 2 of hydrogen as the more probable constitution.

It is a singular example of Dalton's inaccuracy as an experimenter, that he denies "that charcoal, after being heated red, has the property of absorbing most species of elastic fluids." "I made 1500 grains of charcoal red hot, then pulverized it, and put it into a Florence flask with a stopcock; to this a bladder filled with carbonic acid was connected; this experiment was continued for a week, and occasionally

* Introduction to the Atomic Theory, Second Edition, p. 477.

† See Chapter IV.

examined by weighing the flask and its contents. At first there appeared an increase of weight of six or seven grains, from the acid mingling with the common air in the flask, of less specific gravity; but the succeeding increase was not more than six grains, and arose from the moisture which permeated the bladder. Yet carbonic acid is stated to be the most absorbable by charcoal." It is obvious that no accurate result could be obtained from an experiment so conducted. The object of heating the charcoal red-hot, viz. to drive off air and moisture, was defeated by the process of pulverizing it, and bringing it in contact with atmospheric air, in the Florence flask, not to mention the "moisture which permeated the bladder." All exact experimenters have employed compact box wood charcoal, in mass, and have plunged it, when red hot, under mercury, into dry gases standing over mercury. Rouppe and Van Norden had established the large absorbability of several gases in the year VIII of the Republic; and the subsequent experiments of Saussure, made with every care, leave no room for controversy. Having had occasion to repeat them, I can testify to their accuracy. Saussure proves that a volume of box wood charcoal absorbs no less than thirty-five times its volume of carbonic acid gas, and ninety volumes of ammoniacal gas.

It is mainly from this second part of the *New System* that we must gather the materials for adjusting Dalton's rank as an experimentalist. He himself, in the Preface, estimates its value entirely as a repository of "original facts and experiments." In this respect, it will certainly fall much below the expectations of those, who now peruse it for the first time, influenced by his great name in chemical philosophy. It is necessary to confess that he does not appear to advantage in the contest, in which he has here ventured to engage with Davy and Gay-Lussac. Dalton was not great in experimental chemistry. It may be urged that, as a chemist, he was entirely self taught, and commenced his labours at a time when the resources of the experimentalist were scanty and imperfect. Yet there must have been some inherent deficiency in his mental or manual

endowments, disqualifying him for accuracy in experimenting. For his great *contemporary*, Berzelius, created for himself, and through his numerous pupils, for Europe, that system of exact analysis, based upon an infinitude of minute precautions; upon rigid weighings, upon vigilant washings on the filter, upon the greatest attainable purity of reagents; which has raised chemistry to its present rank among the experimental sciences. Davy and Gay-Lussac, too, not many years his juniors,* working simultaneously with him in the same mighty era of chemical progress, devised for themselves instruments and processes of research, susceptible of extreme precision. If we compare the experimental researches of Dalton detailed in this volume, 1810, and in subsequent special chemical memoirs, with the marvellous discoveries revealed in rapid succession in the years 1807—11, by the genius of Davy, and recorded in those masterworks of investigation, his Bakerian Lectures; or with the somewhat later transcendent monographs of Gay-Lussac on Cyanogen, Iodine, and the Compounds of Nitrogen,—eternal monuments of exact and exhaustive chemical working,—we cannot hesitate to admit Dalton's vast inferiority in experimental chemistry. Nature, it would seem, with a wise frugality, averse to concentrate all intellectual excellences in one mind, had destined Dalton exclusively for the lofty rank of a law-giver of chemical science.

In January, 1813, Mr. Dalton communicated to the Manchester Society, "Experiments and Observations on Phosphoric Acid, and on the Salts denominated Phosphates." He concludes, in an Appendix added October, 1817, "that phosphoric acid is constituted nearly of 100 phosphorus and 150 oxygen, the atom weighing 23;" being a compound of 1 atom phosphorus 9 with 2 of oxygen 14. Phosphoric acid is now regarded as a compound of 1 atom phosphorus 32

* Dalton was born in the year 1766; Davy and Gay Lussac in the same year, 1778; and Berzelius in 1779. Though Dalton was twelve years their senior, yet from his having devoted many years to meteorological observations, before entering upon chemical research, these four great masters in science were labouring simultaneously in experimental chemistry, and might have had access to the same forms of apparatus and other external appliances.

with 5 of oxygen 40, and its atomic weight is consequently 72. It is therefore constituted of 100 phosphorus and 125 oxygen; for 32 : 40 :: 100 : 125. Our knowledge of this acid and its salts has been so vastly extended by the elaborate researches of Professor Graham, that there can be no advantage in recurring more fully to the early and imperfect views of Dalton. A memoir "On the Carbonates of Ammonia," read in March of the same year, was scarcely on a level with the chemical knowledge of the day, and has been entirely superseded by subsequent experiments, and especially by those of Henry Rose. Thus Dalton affirms, that Gay Lussac's experiment, showing that 1 volume of carbonic acid unites with exactly 2 volumes of ammonia, "does not appear to have been made by him with extraordinary accuracy, and is not entitled to much credit." Ammonia he regarded as consisting of an atom of hydrogen 1, and 1 of azote 5=6. Consequently, he describes as a subcarbonate, consisting of 1 atom of acid and 2 of *his* atoms of ammonia, the salt now known as a carbonate or mono-carbonate. The constitution of the salt, calculated by him from these atomic numbers, is of course erroneous.

The same volume of the *Manchester Memoirs* (vol. iii. p. 446), contains an elaborate essay on Sulphuric Ether. He describes the modes he employed to separate ether from the alcohol, which passes over with it in distillation, and which seem to have been sufficiently careful. For he obtained an Ether of the same specific gravity ·720, and the same boiling point 95° or 96°, as are now assigned to that fluid. By an earlier experiment in 1805, he had deduced for the specific gravity of its vapour the number 2·65, a fair approximation to 2·586, the number now received. But in this memoir he "thinks 3·1 is probably the nearest expression in two places of figures that can be obtained." He also introduces a correction of the numbers for the force of Ether vapour, published in his first great memoir, which were too low, from the experiments having been made "with an article not of the highest purity." He performed the analysis of ether by firing its vapour mixed with oxygen, in

the proportion of not more than 3 to 10 per cent. of the volume of oxygen, in a Volta's eudiometer. "This method," he observes, "I discovered in September 1803, and have used it occasionally ever since." He employed it with more than his usual exactitude. For he found that ten volumes of ether vapour require about 60 of oxygen, and produce about 40 of carbonic acid. This is rigorously exact, Ether (C^4H^5O)* containing in the state of vapour, 4 vols. gaseous carbon, 5 hydrogen, and ½ vol. oxygen. He also correctly affirms, that the atom of ether may be represented as compounded of 1 atom of water and 2 of olefiant gas; though he miscalculates its weight from conceiving olefiant gas to be constituted of an atom of carbon and one of hydrogen, instead of two atoms of each element, or rather four, as is now believed, viz.,† C_4H_4.

Mr. Dalton's *Memoir on Oil, and the Gases obtained from it by heat,* read October 6, 1820, contains some new facts of importance. He discovered in oil gas the presence of a hydrocarbon, which entered into combination with chlorine, like olefiant gas, but which required for combustion a much larger quantity of oxygen, from 5 to 7 times its volume, and produced from 3 to 4 times its volume of carbonic acid gas. This previously unknown compound he denominated super-olefiant gas. He observes: "In order to form a gas of this character, it would only be required to combine an atom of olefiant gas with 1 of carburetted hydrogen, and to condense them both into the space of 1 atom of olefiant gas. Another supposition might be made of two atoms of olefiant gas united and comprised in the space of one. In this case 100 measures would give 400 carbonic acid, and require 600 oxygen. This supposition would fall within the compass of

* Professor Williamson has inferred from the reaction of iodide of ethyl and potassium alcohol, the superior probability of the views of Gerhardt and Laurent, who double the ether atom, representing it by the formula $C^8H^{10}O^2$. This doctrine is confirmed by the action of iodide of methyl upon the ordinary potassium alcohol, which gives birth to a compound ethyl-methyl ether, and not to a separate ethyl ether and methyl ether. See Dr. Hofmann's *Sixteenth Lecture, on Ether.*

† Hofmann's *Fifteenth Lecture.*

some of the results." In a supplement dated May 1823, after the experiments of my father, which established the existence of a higher hydrocarbon than olefiant in Coal gas, Dalton adopts this last supposition, viz., that the atom of the new gas consists of two of olefiant, as most probable. Thus his superolefiant is, in fact, identical with the bicarburetted hydrogen, discovered by Mr. Faraday, in the liquid formed by compressing oil gas.

An interesting memoir is next in succession, entitled *Observations in Meteorology, particularly with regard to the Dew Point, or quantity of vapour in the atmosphere, made on the Mountains in the North of England from* 1803 *to* 1820. The object he had in view, was to ascertain by actual observation, at various accessible altitudes, whether his theory of the existence of an aqueous vapour atmosphere, distinct from the general atmosphere, and decreasing in density in ascending, in a geometrical progression to increments of height in arithmetical progression, was borne out by fact. "As I had, for some years, been in the habit of allowing myself a week or two in summer for relaxation from professional engagements, and had generally spent the time in breathing the salubrious air of the mountains and lakes near my native place, in the North of England; it was, therefore, an additional gratification to be enabled to unite instruction with amusement. The principal mountain on which the observations were made, was Helvellyn; but most, or all of the other high mountains in the vicinity, have occasionally served my purpose." I had the pleasure of accompanying him, with my father, to the lake district in 1823; and of ascending Helvellyn with him, and witnessing his mode of taking the Dew Point at three stations, in the valley, at Brownrigg Well, and at the summit. From the entire series of observations, made each year, from 1803 to 1823, he concludes "that the quantity and density of vapour is constantly (or with very rare exceptions) less the higher we ascend."

This appears to be the most suitable occasion to introduce the interesting narrative, for which I am indebted to my

venerable friend Mr. Jonathan Otley,* of Keswick, of the numerous successive mountain excursions he undertook in Mr. Dalton's company between the years 1812 and 1836.

"On the 6th day of July, 1812, I first met with Mr. Dalton on Skiddaw. Observing that he carried a barometer, I introduced myself by saying that I had seen a little of the use of a barometer in measuring the heights of mountains, having about three years previously accompanied the Rev. Dr. Pearson to Skiddaw, where I saw its application; and he having left the instrument in my possession, I had afterwards made some experiments with it myself. Mr. Dalton then told me that he had twice, with a guide from Langdale, attempted to reach the top of Scawfell, but the weather at both times becoming unfavourable, the result had not been satisfactory; and that he now proposed to reach it from Wasdalehead: that he expected two friends from Kendal to join him next day, and if I would go along with them they would be glad of my company. Accordingly at noon next day his two friends arrived, and each of us being formally introduced by name, John Dalton, Thomas Wilson, Wilson Sutton, and Jonathan Otley: off we set for Wasdale Head, where we expected to obtain quarters for the night at some of the farm-houses. Passing through Borrowdale we called at the Black-lead mine, where we saw a good quantity of the mineral, as they had recently opened out a productive sop. From thence shaping our course over the trackless mountain towards Wasdale Head, the clouds lowered so fast that we were presently enveloped in a thick mist. And in our anxiety to reach our destination before the early time at which the inhabitants of these vales retire to rest, Mr. Dalton was leading the way at a brisk pace, while the other two gentlemen were not able to keep up; I, in the middle place, had some difficulty in keeping them all within sight, and in such a case it would not have been safe to have been separated farther. An expression of one of the gentlemen, Wilson Sutton, has often since recurred to me: it was, "John! I

* Author of an admirable work "*A Descriptive Guide to the English Lakes and adjacent Mountains*, 7th edition, 1843.

wonder what thy legs are made of!" Having passed the first ridge, and descended a little towards the lower edge of the mist, the first object that came in view was a huge rock, which, from its indistinct appearance, I at first took to be Lingmel Crag, on the further side of Wasdale Head; but on getting a little lower we were undeceived, by seeing the bright silvery stream of the Lisa meandering down the vale of Ennerdale on its way to the lake. Having reached the peaceful vale of Wasdale Head, a place at that time not much visited by strangers, we began to look out for lodgings. At the first house, Mr. Isaac Fletcher's, we obtained lodgings for two, which was very acceptable to our two Kendal friends; Mr. Dalton and I journeying on to Mr. Thomas Tyson's, where we found good accommodation.

In the morning having procured a person to show us the way, we set off between nine and ten o'clock for Scawfell. On our way we stopped at the edge of Wast Water, where Mr. Dalton made some observations of the state of the barometer and thermometer; we then followed the road towards Eskdale till we came to some peat lodges, when we turned to the left up a somewhat regular ascent towards the summit of the mountain. Our guide proved a sleepy one; he had been out fishing all night, and whenever we stopped to make observations, in which he found no interest, he would be taking a nap. The clouds covered the highest peaks of the mountain, but in hopes of their clearing off we continued our march into the mist, still moving forwards till we found ourselves upon the highest point within our reach. Here we waited for some time, now and then having a glimpse of the sun's disk, we were convinced that the cloud was of no great thickness, and it was not long before the sun shone out in full splendor. The barometer which at the edge of the lake stood at 30·2, had now fallen to 27·17. The thermometer varied according to the thickness of the mist from 56 to 60, and after the sun shone out, it rose nearly to 70, which was very high for such an altitude. We now descried a point of land at a short distance, evidently higher than the place where we stood; the space between being filled by an

impenetrable mass of cloud, with a surface so compact, that according to an observation by Mr. Dalton, "one might almost swim over it." On our telling the guide that the further was the point to which we had wished to ascend, he replied: "Ye sed ye wanted to gang to th' top o' Scawfell, and this is't top o' Scawfell however." We then asked what he called the other point, he replied, "The Pikes;" but when we inquired how we were to get to it, he hesitated, and at length he said there was a way across Mickle Door, which some of the shepherds could pass, but he was sure that we should not be able to manage it; and to go any other way would be so far about, that we should not have time to accomplish it before night. We noticed some sheep that we thought were going that way, and that we might have followed them; but he said they were not stinted, and if they found they could not go on, they could come back. After the sun shone out no day could be wished to be finer; we enjoyed ourselves for about two hours, chiefly in agreeable conversation, without committing many of our remarks to paper, and about four o'clock we began to descend, and returned to our quarters for another night.

Having often heard mention of a fine spring of water on the very top of Gable, or as it is called here, "The Great Gavel," we made some inquiry of Mr. Tyson, who informed us that it was never known to be dry but once on the occasion of a fox hunting, when the dogs lapped it dry. We all felt desirous to ascertain the truth of this subject, but as Mr. Dalton had fixed to be at Low-wood on the following evening, he had not time to visit it; but it was concluded that I should go that way to enable me to report. So after keeping company to Stye Head, these gentlemen turned to the right by Sprinkling Tarn, and Angle Tarn to Langdale; I, on the other hand, attacked Gable; and although we had been disappointed in reaching the highest point of the region, the philosopher was amply compensated, not only by the extensive views of the country, but by the opportunities afforded of studying the constitution of the atmosphere, and the transition from cloud to sunshine, and from sunshine to

shadow in those lofty regions. And it was particularly interesting to watch the formation of mist in Wasdale Head, its condensation into clouds in passing over the mountain, and their dissolution on reaching Bonowdale. On arriving at the top of Gabel I did not meet with an actual spring as it had been called, but I certainly found a well or cistern in the rock about six inches deep, and containing about two gallons of clear water of the temperature of 71°, that of the air in the cloud being 65°, and in the sun 70°, an unusually high temperature for a place 2,900 feet above the level of the sea. No water being seen to enter this well, or to run from it; I lowered it sensibly by drinking, and it did not regain its level while I remained; I have since learned that it was dry in the summer of 1826.

On the following year 1813, July 6th, Mr. Dalton called on me to accompany him to Skiddaw. He brought a large theodolite which he had taken to the top of Helvellyn the day before. He engaged a pony and a boy to attend it to carry up the instrument, which was employed in making a circuit of observations of the bearings, elevations, and depressions of the several mountains by which we were surrounded. On his showing me the circuit of mountain observations that he had made on Helvellyn, I conceived that he had interchanged the names of two of them. As he could not submit to leave the point undecided till another season, it was agreed that I should accompany him to Helvellyn on his return from Cockermouth and Eaglesfield. Accordingly about three days afterwards we ascended Helvellyn for the first time in company, and the mistake was soon rectified. In a letter dated July 23rd, I was favoured with a list of the bearings and elevations of several mountains as taken from Low Wood Inn, and also from a hill near Lancaster. He added that "A cast of the mountain country in plaster of Paris would be a very grand thing in which the magnitudes, figures, and bearings of the mountains were properly laid down." In a letter dated February 28th, 1814, he says, "I should wish on some future occasion to have the mountain views from the Old Man, Great End, and the Pillar; they would be grandly diversified

from those three points." January 28th, 1815. He communicated the flattering intelligence that I had been elected a corresponding member of the Literary and Philosophical Society of Manchester; and in July of that year we had a little fishing excursion on Derwent Lake. We examined the place of the Floating Island, which had not at that time risen to the surface, as it did shortly after. We filled some bottles with gas issuing from places where we probed it with a pole. September 15th, I had a letter informing me that he had analyzed the gas, and found it to be half carburetted hydrogen and half azote, with a slight portion of carbonic acid gas. He had sometimes given his opinion that a view from the Pillar would exhibit the mountains in a favourable point of view, and he now expressed a wish to ascend it. Accordingly in the afternoon we took a walk to Buttermere, in order to be so far on our way in the morning; but the day proved unfavourable, and we waited for several hours without seeing, as Mr. Dalton expressed it, "as much blue sky as would make a pair of breeches," and were obliged to relinquish the Pillar for another season.

About the usual time in 1816, Mr. Dalton arrived at Keswick, accompanied by his friend and companion of some former excursions, the Rev. Mr. Lamport, then of Lancaster; and on the 9th July, we three took the road to Buttermere, in expectation of surmounting the Pillar next day. The morning did not appear very promising; but, however, we resolved to make the attempt. We passed Scale Force, and over Floutern, towards Ennerdale. Mr. Lamport had a pony, which gave us some trouble in crossing the bogs; and when we allowed it to choose its own path, it seemed inclined to walk off and leave us. From the state of the weather, it long appeared doubtful whether we should be able to accomplish our object; and, as we reached the vale of Ennerdale, a shower came on, which caused us to look out for shelter, and we made towards a house which proved to be empty; and here we were joined by some shepherds, who, returning from the fell, came in to escape the rain. Here we remained some time in uncertainty; and Mr. Lam-

port, less strenuous than Mr. Dalton, gave up the object and left us, having consigned a piece of cold tongue, and a small flask of brandy to our use, in addition to our own stock of provisions; and promised to call at Ennerdale Bridge to bespeak our lodgings. The only inhabited house between us and the Pillar was Gillerthwaite, which we understood was at that time occupied by a shepherd of the name of Birkett, and his mother; and we were in expectation, if the day cleared up, of engaging this man as our guide. However, while we were waiting, this same person happened to be passing with a horse and cart. We explained our case to him; but he told us, that he was engaged going to Lamplugh Cross for slate to make some repairs, but that an aged shepherd of the name of John Simpson, who was there at that time, would answer our purpose very well. It appeared very strange to us, that slate, quarried at Honister Crag, should have to be carried all the way by Lamplugh Cross, making the journey about four times the original distance. Having agreed with Simpson, and the weather beginning to clear up, we started anew, and, calling at Gillerthwaite, we found Mrs. Birkett baking oat cakes in the Westmoreland fashion; and although we were not unprovided with refreshments, we partook of her oat-cake and excellent butter and milk with a high relish, as being the produce of that sequestered region. About three o'clock, we, not without some difficulty, crossed the Lisa, there being no regular stepping stones, and slanted up the mountain to the *Wind Yat*, the gap between the Pillar and adjoining mountain. Here we looked down the branch of Mosedale into Wasdale Head; after this we had to climb a steep ridge; and Mr. Dalton, anxious to reach the summit, was leaving us behind. Simpson, who had more experience, says, "Let him be going, we'll catch him at some turn." About five o'clock, we reached the top of the Pillar, or rather Pillar Fell, the Pillar itself being a projecting rock on the side towards Ennerdale, which had long been considered inaccessible; but we have since learned that it was ascended by an inhabitant of Ennerdale in 1826, and since that by two or three others.

During the short time we could remain upon the hill, our chief object was making such observations as might enable us to place the Pillar in its place on the map, which had hitherto been inaccurate. Returning over a smooth piece of ground, we were shown three large stones, where men, women, and boys were wont to exercise their strength in lifting. He must be a strong man who lifts the largest: I was content with the smallest. Before reaching Ennerdale Bridge, we parted with our friend John Simpson, well pleased with his attentions, and his anecdotes of a shepherd's life. He was anxious to reach home, having been on the mountains ever since day-break. And on arriving about half-past nine at the inn, we found that Mr. Lamport had executed his commission; but that only one bed had been reserved for us; and the landlady seemed surprised that we should want more. However, we were gratified that the object of our journey had been accomplished: and we were not fastidious about entertainment. Mr. Dalton having taken the trouble to analyze some of the minerals of this district, he naturally felt a curiosity to see the places where some of them were procured; accordingly, in 1817, we undertook an excursion with that view. We looked at the copper mine at Dalehead, in Newlands, and some iron and lead ores near Honister Crag. We had provision for the day prepared by Mrs. Lancaster, of the Hare and Hounds, in more than sufficient quantity, but in not due proportion. In a meal out of doors, bread is generally eaten along with other things, and on opening out our evening repast, we found a deficiency of that article; all we had left was cold lamb and butter, with a little spare salt, which we left as a medicine for the sheep. This circumstance my late lamented friend endeavoured to call to my recollection, I think, the last time that I saw him.

1819, *July 7th.*—In order to ascertain what were the portions of the ridge beyond St. John's Vale, that he had been in the habit of seeing from his native place, Eaglesfield, we took the Cockermouth road to the top of Whinlatter, and then turned up along the ridge, where we

could have a view both to the vale of Keswick and also about Cockermouth and Eaglesfield. And when we reached Grisdale Pike, we had a splendid view of the north-western parts of Cumberland, and a portion of Scotland, and a romantic view of the glen of Hobcarton and its crags near us.

1820.—After often calling to mind our unsuccessful attempt, in 1812, to reach the greatest height, not only in this district, but in England, it was determined that we should give it another trial. Accordingly, on the 17th of July, we walked up to Rosthwaite, in Borrowdale, where we took up our lodgings to be so far on our way to attack the hill in the morning. Since our former attempt, I had made myself acquainted with the different points of the mountain, as well as the way to reach it; so that we wanted no other guide. Indeed, none of our professing guides had, at that time, made themselves at all acquainted with it. The weather was tolerably favourable, though not entirely cloudless; and we took our way very deliberately, making observations as we went along. We arrived at the desired point about half-past eleven; a cloud rested upon the summit at the time, but we had not long to wait before it cleared off and allowed us as complete a circuit of observation as could be desired. Having recognized as old acquaintances the mountains all around, our attention was attracted to objects more at home. From one point, Mr. Dalton was pleased to distinguish the residence of his friend Mr. Knight of Papcastle. We distinctly saw the houses at Troutbeck Bridge; and, from a lower point, about 150 yards distant, we discerned the inn at Low-wood, which Mr. D. had frequently made his quarters. This monarch of the mountains has been called by different names in the several valleys that it overlooks, which has led to some misunderstanding. In Langdale and Eskdale, it was called Broadcrag, in Wasdale Head the Pikes, and in Nether Wasdale the lower point is called Scawfell. In a published volume of the trigonometrical survey, it is called Scaw Fell high point, or Scaw Fell higher top; and a letter from Lieut. Murphy was dated "Camp on

Great Sca Fell, 2nd Sept., 1826;" and it is now most generally known as Scawfell Pike.

As the Gable appeared so eminent from many points of view, independent of the natural curiosity of its well, Mr. Dalton had often expressed a wish to ascend it. Accordingly, on 11th July, 1821, we walked up Borrowdale as far as Seatollar, and then took the road towards Buttermere; near the house, turned to the left, leaving Honister Crag and Fleetwith Pike on our right; in one place we passed a piece of rock or a large stone, on which was a well or cistern, containing several gallons of water, notwithstanding that the weather had for some time been droughty. As we crossed the path we had taken in 1812, we came in sight of Ennerdale Lake; and we scrambled up the glen between Gable or Great Gavel, and the Green Gavel, reaching the summit about four o'clock. Thermometer 50° to 56°. The Barometer at Keswick was 29·916, here it had fallen to 27·15, and the long fit of dry weather had reduced the water in the well to the depth of two inches. This well has not been of such repute, since it was interfered with by the erection of the pile by the ordnance surveyors. We descended by Sty Head to Wasdale Head; but on reaching our old quarters at Mr. Tyson's, we found that their beds were engaged; so we returned to Christopher Fletcher's, where we were concerned to find that Mr. and Mrs. Fletcher had gone to a clipping at Eskdale, and we had some difficulty in prevailing upon Mrs. Fletcher's sister, who was left in charge of the house, to accommodate us with beds. However, after having succeeded, we found ourselves quite comfortable, and in the morning, after collecting some rock specimens, we journeyed leisurely to Keswick.

1822, *August* 8.—An excursion to Helvellyn, in which there was nothing unusual to record. On the following day, we clambered up the steep mountain called the Stile, in the junction of Cumberland and Westmorland, with a view of tracing a line between Skiddaw and Lancaster; but the atmosphere was not sufficiently clear for our purpose.

1823, *July* 6.—Mr. Dalton informed me by letter, that

he was on his route, accompanied by Dr. Henry and his son; and they would be glad if I could meet them on Helvellyn. Accordingly I went on the road to Wythburn, where I learned that Mr. Dalton and Mr. C. Henry, had gone on the mountain, and left Dr. Henry at the Nag's Head, so I waited with him till their return.

1824, *July* 8.—I had accompanied Professor Sedgwick over Armboth Fell to Wythburn, where by accident we met with Mr. Dalton; a meeting alluded to by professor Sedgwick, in his address to the British Association at Cambridge, in 1833, on announcing the pension of 150*l.* a-year, allowed by government to Dr. Dalton. The two diligent cultivators of science appeared mutually gratified by the interview, and spent a most pleasant evening. I had the pleasure of joining them at breakfast, when I recollect the professor rather touched the feelings of our hostess at Nag's Head, by asking if it was tea or coffee she had brought in for us.

1825, *July* 20.—Our excursion for this season was on Skiddaw, at three P.M. thermometer 68°, dew point 58°, at seven P.M., on Castlehead, thermometer 63°. Skiddaw was coated by cloud in a peculiar manner. On 19th had been thunder, thermometer in the shade 86°, in the sun 108°.

About the middle of September, the floating island being above water, I collected some gas from it, a bottle of which I sent, along with another bottle, which had been kept since 1815. January 15th, 1826, I received a letter, stating that he had been confined by the worst cold he had ever had, which had for a time greatly affected his hearing; he says, "I examined the bottles of air; the new and the old were exactly of the same quality, a circumstance to me very surprising. I examined the air from Helvellyn and Skiddaw. The former was considerably inferior in oxygen to the air below; and the latter a little but much less than the other; you will remember that the atmosphere was changing much about that time; from the odd appearance of the clouds the evening we left Skiddaw."

1827, *August* 22.—On Helvellyn, with a new barometer

of my own construction on trial, but found the tube rather imperfect.

1829.—An excursion to Saddleback to notice the change in the mountain forms, and particularly to look at the tarn, of which so much had been said. We could not trace any volcanic appearances as mentioned; nor any reflection of the stars. Next day, with the intention of going over the mountain to Patterdale, we took the coach to Legberthwaite, and in ascending towards the Sticks, we were entertained by our old friends the mountains on the west, as they successively made their appearance over the long and uninteresting ridge of Castlerigg Fell, betwixt us and Derwentwater; often noticing the barometer as we advanced in altitude. We visited the exterior of the extensive lead mines at Greenside, passed down the vale of Glenridden, to the inn at Patterdale, where we slept. I found in the morning, that I might have set all the bells in the house a ringing without moving from my bed. In the morning we took the Ambleside road to the top of the pass of Kirkstone; where an inn has been since that time erected; here we left the road, and scrambled up the red screes, to the top of the Fell, which is sometimes called Kirkstone, I call it Scandale Fell; and we descended on the Scandale side to Ambleside.

1830, *July* 13.—Took a conveyance to Dunmail Raise, and ascended by the skirt of Seatsandal to Grisedale Tarn, from whence we had steep climbing to the top of Fairfield; we saw eight lakes and tarns, and descended to the Swan at Grasmere.

1831, *July* 9.—From Low Wood Inn we ascended to the Pike of Wansfell. We had some trouble in getting through the fields to Skelgill; but on the open common we seldom met with any difficulty. From the summit we had pleasing views of Windermere, and other scenery.

1832, *September* 19.—I was with Dr. Henry on Skiddaw, when water boiled at 206½°. Ther. on summit, 44°; bar. at Keswick, 30·06.

1833, *July* 6.—Being on a journey to Kirkby Lonsdale, I was told at Wythburn that Mr. Dalton and three ladies

were upon Helvellyn. I left the coach, and went a little way up the hill to meet them. We remained all night at Wythburn; and in the morning they accompanied me on my road towards Grasmere as far as Dunmail Raise. By the way he related some of the proceedings at the meeting of the British Association in June the year before, where they robed him in a scarlet gown, a colour that his eyes did not recognize, as a Doctor of Civil Law; and also at Cambridge in the present year, when his pension of 150*l.* was announced, which he feared would endanger his tranquillity. The before-mentioned are not the only occasions, before this time, that I have had the pleasure of accompanying the venerable philosopher among his favourite mountains; but I must now advert to the concluding one.

1836, *July* 5.—We left Keswick in the afternoon by the coach in hopes of reaching Helvellyn in the evening. There had been some thunder, but it seemed to be clearing off, and as soon as we arrived at Wythburn we lost no time in commencing the ascent; but before we had got half way up the thunder recommenced, accompanied with rain; so that we found ourselves under the necessity of retreating. Next morning the storm had passed away, the weather was settled, and we started again. Dr. Dalton had provided a large bottle, which I took up in a bag, with the intention of bringing down a larger quantity of air to experiment upon, than on any previous occasion. After making his accustomed experiments at Brownrigg Well, we filled the bottle with water, which before we arrived at the summit had got too warm to exhibit the dew point without some saline mixture. Pouring out the water we had the bottle filled with air, which we took down; but I never heard the result. Two *friends* came up with a guide from Patterdale; and we proffered our assistance in conducting them to Wythburn. We deviated to the place where Mr. Marshall had placed a rain gauge, but I understand it was soon demolished by some mischievous person. Descending by Bursett Crag, we had only time to take a little refreshment at the Nag's Head when the coach came up, by which the Doctor and two

strangers departed southwards, while I walked to Keswick. It was on this occasion first explained that the Doctor and I were both born in 1766; I in the first quarter, he in the third: and it was also intimated that his pension was to be doubled.

Dr. Dalton usually travelled by stage as far as the coach served his purpose; the rest of his journeyings were chiefly accomplished on foot. He used to say that a little mountain exercise brought into play a certain set of muscles which would otherwise turn rigid and inactive. He was active and persevering in climbing a mountain; especially when he came in sight of the goal, there was no keeping pace with him; in descending, or on rough ground, I was fully his equal; my stronger shoes enabled me to venture more freely. The barometer which he carried was of the most simple construction; yet its action was more intelligible than some fitted up in a more expensive way. His eyes, though subject to some defects, were very exact in estimating small divisions of space. His mode of calculating altitudes generally came out something higher than what has subsequently been given in the ordnance survey; but for his purpose the greatest exactness was not required. In later years he declined bringing his barometer, as we had the privilege of using one belonging to the Rev. Dr. Pearson, and afterwards one of my own construction. He was never averse to taking Matthew Jopson's advice in taking a little brandy to mix with the water of Brownrigg Well; but he was very abstemious in using it. Although these excursions have been undertaken chiefly as recreations, they have been not without their use; they assisted in the investigation of the constitution of the atmosphere; and we have been enabled to make a step in advance of our predecessors in the geographical delineation of the district. Although the Doctor always treated me as a companion, he would never permit me to go without some pecuniary remuneration. I must not say for loss of time, as no time could be said to be lost that was spent in his company, he was so affable and communicative. When, on the last-mentioned occasion I would have declined

what he offered, he said I must take it, it might probably be the last. And as far as regarded mountain excursions, or journeying in company, so it was. I saw him at Keswick two or three times after that: but still with a kind of melancholy pleasure.

[Compiled in 1844, and copied, with slight variation, August 15, 1853, by Jonathan Otley.]

CHAPTER VII.

OFFER OF AN APPOINTMENT IN THE POLAR EXPEDITION—ELECTED CORRESPONDING MEMBER OF THE FRENCH ACADEMY OF SCIENCES—FELLOW OF THE ROYAL SOCIETY—VISITS PARIS, 1822—EXTRACTS FROM HIS JOURNAL—MR. DOCKRAY'S LETTER AND ACCOUNT OF A DINNER AT LAPLACE'S — ROYAL MEDAL—DAVY'S ANNIVERSARY DISCOURSE—PUBLISHES 1827 VOLUME II. OF "NEW SYSTEM."—FOREIGN ASSOCIATE OF THE FRENCH ACADEMY, 1830—ATTENDS THE MEETINGS OF THE BRITISH ASSOCIATION AT YORK AND OXFORD — RECEIVES THE HONORARY DEGREE OF D. C. L. AT OXFORD—PENSION, 1833—DOUBLED, 1836 — PROFESSOR SEDGWICK'S SPEECH AT CAMBRIDGE—STATUE BY CHANTREY—DEGREE OF L.L.D., EDINBURGH—PRESENTATION AT COURT—MR. BABBAGE'S LETTER.

Early in the year 1818, Mr. Dalton was invited by Sir H. Davy, in the following letter, to join the expedition to the Polar regions, then about to sail under the command of Sir John Ross:—

"23, Grosvenor Street, Feb. 12, 1818.

"My dear Sir,—You have probably heard of the expedition which is preparing for investigating the polar regions. The Royal Society, charged by the Admiralty with the scientific arrangements of this voyage, is very desirous of making the most of so interesting an opportunity of investigating many important objects relating to meteorology and the theory of the earth.

"They have obtained from the Admiralty the power of recommending a natural philosopher to go on this expedition; and it has occurred to me, that if you find your engagements and your health such as to enable you to undertake the enterprise, no one will be so well qualified as yourself.

"Probably 400*l.* or 500*l.* a-year will be allowed during the voyage. It may last from one to two years; but most

probably only three or four months: in all events, it is believed, that the whole year's salary will be allowed.

"This plan may not in any way fall in with your views. At all events, I am sure you will not be displeased with me for inquiring if such a proposition will be acceptable to you; and impute this letter to the high respect I have for your talents and acquirements, and the desire that they may be brought into activity, and estimated and rewarded as they ought to be.

"I am, my dear Sir, very sincerely yours,

"H. DAVY."

Dalton replied immediately:

"RESPECTED FRIEND,—In reply to your favour of the 12th instant, I may observe that the novelties of an enterprise similar to the intended one, can hardly fail to present much gratification to individual curiosity, and many interesting opportunities for observation of natural phenomena, particularly of meteorology, which certainly operate as strong inducements for me to engage in it; but on the other hand to quit the regular habits of a sedentary life for a sea-faring one, and that on a voyage of uncertain duration, and of more than ordinary risk, with the prospect of a cold, or at least, a desultory climate; add to these the interruption of my chemical investigations which I have not yet been able to close,—these together outweigh with me any inducements which the proposed scheme can offer. I must therefore decline the acceptance of the proposition.

"At the same time, I would have it to be understood that I feel gratified by the communication of your favourable opinion of my scientific labours; and it will afford me great pleasure if I can in any other way contribute to the successful issue of the expedition, especially as far as it may be connected with the promotion of science.

"I remain, with high esteem,

"Your obliged Friend.

"Sir Humphry Davy, "JOHN DALTON.

"23 Grosvenor-street, London."

Dalton was elected a corresponding member of the French Academy of Sciences in the year 1816. He thus announces his election to his brother:

"October 30, 1817.

"I have just received a visit from M. Biot, the French Academician, who has been in the Shetland Isles, making observations on the pendulum, &c.; he has lately written on the expansion of liquids, in reference to my essays. He has been active in measuring an arc of the meridian in Spain, &c., He is a very intelligent gentleman and pleasant companion. I do not know whether I mentioned that the French Academy of Sciences lately elected me a corresponding member on the subject of chemistry, an honour that has been conferred only on one other person in this kingdom, I believe, namely, on Dr. Wollaston, Secretary to the Royal Society. I received the diploma a few months ago."

It was not earlier than March 1822, that he was received into the Royal Society. This apparently tardy recognition of Dalton's extraordinary services to science cannot, however, be fairly urged as an impeachment of the justice or discernment of the governing body of the Royal Society. For, as respects British subjects, it has never been its province to select spontaneously distinguished cultivators of science for the honour of its fellowship. Its statutes require persons, ambitious of such distinction, to be proposed as candidates, by Fellows who are able to give testimony of their possessing due qualifications. If blame attaches to any one, it must fall on the personal friends of Dalton in the Royal Society. Dalton has, however, himself recorded, "In 1810 I was solicited by Davy to offer myself as a candidate to the Royal Society, but I declined it. In 1822 some of my friends proposed me without my knowledge; I do not know who they were until this present time, but it will be upon record, I suppose. I was elected and paid the usual fee."—Addenda to an Essay on the phosphates and arseniates, 1840. I am inclined to conjecture, that the amount of the admission fee and composition may have deterred Dalton in earlier

years from permitting Davy to propose him for the Fellowship.

In the summer of that year, 1822, he visited Paris in company with Mr. Benjamin Dockray and Mr. W. D. Crewdson. He had unfortunately preserved only very brief notes of this interesting journey. The first person upon whom he called was M. Brèguet,* the celebrated mechanician, and a member of the Institute, merely with the object of placing in his hands a watch constructed by Brèguet, that required some repairs. When M. Brèguet learned the name of his visitor, he welcomed him with the liveliest enthusiasm, and immediately engaged him and his two companions to dinner, where they met M. Arago, M. Fresnel, and other distinguished persons. "Saturday, July 6th, received a visit from two Swedish chemists from Abo, in Finland, pupils of Berzelius, Bonsdorf, and Nordenskiold. Visited the venerable Abbé Gregoire. 7th Sunday, Attended the service at the British Ambassador's chapel. From one to two hundred present, chiefly English, and more than half ladies. Very genteel and attentive congregation, good sermon well calculated for Paris, on the evidences of Christianity. After 4P.M. took coach with companions for Arcueil, to dine by invitation with the Marquis La Place and Lady. Met Berthollet, Biot, and Lady, Fourier, &c. &c. A most agreeable and interesting visit, and a beautiful place. Monday, 8th July, walked down to the arsenal, saw Gay-Lussac for half an hour; went to the Jardin du Roi; saw the wild beasts and the anatomical preparations, &c.; took coach home, and then went to the Institute, about 100 persons present; was introduced by Biot and placed in the square adjacent to the officers; was announced by Gay-Lussac (as president) as a corresponding member (English) present. The sitting was from 3 to 5 o'clock. After my

* M. Brèguet Fils wrote to Dalton:—"Mon père a vu M. de la Place ce matin au Bureau des Longitudes. Il désire beaucoup vous voir et vous engage pour Dimanche prochain à aller avec vos deux amis, diner chez lui à Arcueil près Paris. M. Arago a été enchanté de l'espoir de vous rencontrer, demain chez nous à 5 heures precises; car nous comptons toujours sur votre promesse de faire ce que vous pourrez pour cela."

announcement, my two companions were introduced to the same bench during the sitting. Sunday, 14th, Gay-Lussac and Humboldt called and spent an hour on meteorology, &c.; took coach to Thénard's, breakfast à la fourchette with him, family, and Dr. Edwards; went to the laboratory near M. Biot's, and saw a full set of experiments on the deutoxide of hydrogen, most curious and satisfactory. M. Thénard then went with us through the laboratory; shewed us the new theatres for chemistry, physique, &c., and then went to M. Ampères, who had previously prepared his apparatus for showing the new electro magnetic phenomena. Saw a set of these experiments, which, with the aid of Dr. Edwards, were made intelligible to me. 15th. Took coach to the arsenal; spent an hour with Gay-Lussac in his laboratory; saw his apparatus for specific gravity of steam, vapours, &c., also M. Welter's, the improver of chemical distillation, &c.; walked to the Jardin du Roi; *déjeuner à la fourchette* with Monsieur and Madame Cuvier and youngest daughter. M. Cuvier went with us to the museum, and accompanied us for some time, and then left a gentleman to attend us through the museum, being himself engaged, but occasionally meeting us; spent two hours in the museum,—the most splendid exhibition of the kind in the universe,—it beggars description. Left after 2, and took a coach to the Institute; took a cup of coffee, &c., and then entered the library; saw and spoke to M. Edwards, Biot, Cuvier, Laplace, Berthollet, Brèguet, &c.; entered the Institute; heard papers by Edwards, Biot (on the Zodiac de Denderah) Fourier, on the population of Paris, after which, notice was given for strangers to withdraw, when Gay-Lussac called to me to stay, if I chose, being a member, which I did. The business was about election of members, and lasted nearly half an hour, after which we broke up; saw M. Pelletan on coming out, who kindly inquired of me, my health, &c.; went with Vauquelin in a coach to dine, when my companions met me; saw M. Payant, a young chemist of promise."

It is, perhaps, worthy of record, that he was not deterred by any sectarian principles from visiting both the Italian Opera and the Théatre Français.

Mr. Dockray, one of his fellow travellers, and now residing in *Dalton* Square, Lancaster, has obligingly favoured me with the general impression remaining on his mind, after the lapse of thirty years, of his sojourn in Paris with Dalton. "He was so entirely devoted to the interests of science, that, except in the intervals of relaxation, when he entered thoroughly into whatever interested those around him, his habitual manner, his caste of character, had acquired what may be considered a congenial equability that seemed to exempt him in great measure from personal influences, as well as from all undue susceptibility to considerations regarding his own individual repute or interest. I entirely believe, that the original and memorable experiments, which Thénard and Ampère severally exhibited before him, gave him as much pleasure, since they were instances of success in eliciting the great truths of nature, and intimated still onward views to be afterwards pursued, as if they had been the result of his own researches. All his friends well knew that, notwithstanding his eminent position and the recondite nature of his studies, no one could more readily find interest or amusement in common things and incidents; and that no one's attention could more readily be engaged by the sort of novelties which occur in foreign travel. I was particularly struck by observing the impression made on Dr. Dalton by the solemnities of Roman Catholic worship, and the evident sincerity of profound devotion which he saw there; and I do not doubt it was to him a page of human nature, which, till then, he had never had an equal opportunity of witnessing. Second, I think, only to this, for impressiveness of novelty, was the gallery of the Louvre. I do not doubt, but that he felt, there was, in the masterpieces of art which he saw there, a new world of interest and wonder, on which he would gladly have had the opportunity of longer meditating." Mr. Dockray has also sup-

plied me with the following graphic narrative of the memorable dinner at Arcueil:—

"July 7, 1822.

"At four in the afternoon, by a coach with Dalton to Arcueil, La Place's country-seat, to dine. Engaged the carriage to wait for our return at nine. On alighting, we were conducted through a suite of rooms, where, in succession, dinner, dessert, and coffee tables were set out;—and onwards through a large hall, upon a terrace, commanding an extent of gardens and pleasure-grounds. There was a sheet of water in front, and a broad spreading current pouring into it from some rocks, where was seen a sculptured figure—an antique—found in the locality, representing the genius of the place. It is in these grounds that are still remaining the principal Roman works near Paris,—the vestiges of Julian's residence, as governor of Gaul. Avenues, parterres, and lawns, terraces, and broad gravel walks, in long vistas of distance, are bounded by woods and by higher grounds. As yet we had seen no one, when part of the company came in view at a distance: a gentleman of advanced years, and two young men. Was it possible not to think of the groves of the Academy, and the borders of the Ilyssus? We approached this group, when the elderly gentleman took off his hat, and advanced to give his hand to Dalton. It was Berthollet! The two younger were La Place's son, and the astronomer royal—Arago. Climbing some steps upon a long avenue, we saw, at a distance, La Place walking uncovered with Madame Biot on his arm; and Biot, Fourier, and Courtois, father of the Marchioness La Place. At the front of the house, this lady and her grand-daughter met us. At dinner, Dalton on the right hand of Madame La Place, and Berthollet on her left, &c. Conversation on the zodiac of Denderah and Egypt, Berthollet and Fourier having been in Egypt with Napoleon; the different eras of Egyptian sculpture; the fact that so little at Rome—of public buildings—is earlier than Augustus, &c. After dinner, again abroad in the beautiful grounds, and along the reservoir and aqueduct of Julian.

These ancient works, after falling very much into decay, were restored by Mary of Medicis. Dalton, walking with La Place on one side, and Berthollet on the other, I shall never forget. Such men, in their personal attentions, respect in each other the dignity of science herself—the great interpretress of nature, and leading star of civilization; something which is beyond the honoured individual, which yet attends him, impressing a sense of homage that is elevating to him who feels it. La Place is an uncommon union of simplicity of manners and an essential dignity of character. His collected and serene air realizes to the observer the tranquillizing influence of philosophy. We may well conceive, that such a man feels for the interest and honour of science something like a religious regard. At the Institute a few days before, an instance of behaviour in La Place was a striking exemplification of this remark."

The enjoyment and advantage of his stay in Paris were greatly enhanced by the friendly attentions of Dr. Milne Edwards, who kindly acted as interpreter between him and those of the French savans who did not speak English. Dalton was always accustomed to mention Dr. Edwards in terms of grateful regard; and appears to have maintained some intercourse with him by correspondence. In writing, Feb. 12, 1825, to introduce the family of Mr. Taylor to Dr. Edwards, he says :—" Permit me to return you my best thanks for your valuable essays *De l'Influence des Agens Physiques sur la Vie*. I have not had time to peruse all of them with that attention which they merit; but have seen enough to satisfy me that the subjects are treated with great skill and infinite labour in the experimental department. I regret that I have no recently printed essay to transmit to you. I have been working lately on the heat and light yielded on the combustion of different gases. The heat seems nearly as the oxygen; but the light is subject to various modifications, which I have not yet completely ascertained."

In the year 1825, His Majesty George IV, announced to the Royal Society his gracious intention of founding two

annual prizes, each of the value of fifty guineas, to be at the disposal of the President and Council of that learned body. Sir H. Davy, in his anniversary discourse 1826, made known their award of the first prize to Mr. John Dalton, "for the development of the chemical theory of Definite Proportions, usually called the Atomic Theory, and for his various other labours and discoveries in physical and chemical science." He justly affirmed, that "to Mr. Dalton belongs the distinction of first unequivocally calling the attention of philosophers to this important subject. Finding that in certain compounds of gaseous bodies, the same elements always combined in the same proportion; and that when there was more than one combination, the quantity of the elements always had a constant relation, such as 1 to 2, or 1 to 3, or to 4, he explained this fact on the Newtonian doctrine of indivisible atoms, and contended, that the relative weight of one atom to that of any other atom being known, its proportions or weight in all its combinations, might be ascertained; thus making the statics of chemistry depend upon simple questions, in subtraction or multiplication, and enabling the student to deduce an immense number of facts, from a few well authenticated, accurate, experimental results. Mr. Dalton's permanent reputation will rest upon his having discovered a simple principle, universally applicable to the facts of chemistry—in fixing the proportions in which bodies combine, and thus laying the foundation for future labours, respecting the sublime and transcendental parts of the science of corpuscular motion. His merits in this respect resemble those of Kepler in astronomy. . . . . Mr. Dalton has been labouring for more than a quarter of a century, with the most disinterested views. With the greatest modesty and simplicity of character he has remained in the obscurity of the country, neither asking for approbation, nor offering himself as an object of applause. He is but lately become a Fellow of this Society, and the only communication he has given to you, is one, compared with his other works, of comparatively small interest; the feeling of the Council on the subject is therefore pure. I am sure he will be gratified by

this mark of your approbation of his long and painful labours. It will give a lustre to his character, which it fully deserves; it will anticipate that opinion, which posterity must form of his discoveries; and it may make his example more exciting to others, in their search after useful knowledge and true glory."*

In August 1827, appeared Part I of the second volume of the *New System*. It was printed between the years 1817 and 1821, with the exception of the Appendix, one sheet of which was printed at the end of 1823; but no addition was afterwards made till May 1826. This volume is devoted to the Metallic Oxides, Sulphurets, Phosphurets, Carburets, and Alloys. At the period of its publication, it was far from adequately representing the existing state of chemical knowledge, and is now entirely superseded. Dalton can never be regarded as an authority in the details of chemistry. The Appendix constitutes the only interesting portion of this volume, as containing the matured or modified views, held by him in 1827, respecting many of the great questions, with which he had grappled in earlier life. We have already quoted from it, the modifications, if not the absolute surrender, of his views on temperature, which were induced by the researches of Dulong and Petit. In special chemistry, he relinquishes his notion that Fluoric Acid was probably constituted of two atoms of oxygen and one of hydrogen; subsequent experience having shown "that deutoxide of hydrogen, though it can be formed synthetically, is not the same thing as fluoric acid. . . . . I had no small satisfaction in 1822, when at Paris, in being obligingly favoured by M. Thénard with a view of the process of the formation, and of the more distinguishing properties of this singular liquid." He had still, however, not cordially adopted Sir H. Davy's view of the constitution of Fluoric Acid, "the nature of which," he says, "is still enveloped in obscurity." Nor had his early prejudices, respecting Chlorine, been uprooted, by the force of experimental evidence, assembled

* Sir H. Davy's *Six Discourses*, pp. 125—131.

by Davy, in his successive Memoirs of 1810, 1811, and 1818, and which, long before the year 1827, had commanded the admiring assent of all other European chemists. Thus, after describing the "three notions which have been submitted to the public in the last twenty years in regard to the nature of muriatic acid," he observes, "it is not intended here to enter into a discussion of the arguments and facts adduced in support of the different conclusions. More experience must be had before all the doubts and difficulties are removed from the subject," p. 315. In recurring to the consideration of Potassium and Sodium, he assents, though not unreservedly, to the doctrine that they are simple metals. And in some concluding remarks "on the principles of the atomic system of chemistry," he thus persists, in 1827, in withholding his entire acquiescence from the then universally received doctrine of volumes. "The combination of gases in equal volumes, and in multiple volumes, is naturally connected with this subject. The cases of this kind, or at least approximations to them, frequently occur; but no principle has yet been suggested to account for the phenomena: till that is done, I think we ought to investigate the facts with great care, and not suffer ourselves to be led to adopt those analogies *till some reason can be discovered for them.*" The Appendix terminates with a new or "reformed" table of atomic weights. It is amusing to observe, with what tenacity the veteran philosopher clings to his early determinations, though unanimously rejected by all living chemists. Thus oxygen still figures as 7; nitrogen as 5 + or 10?; carbon as 5·4; sulphur as 13 or 14; and phosphorus as 9.

In the year 1830, the French Academy of Sciences raised Dalton from the class of Corresponding Member, to the rank of one of its eight Foreign Associates, the highest station it has to bestow, and universally regarded as the crowning distinction in European science. In the words of Cuvier, in his *Eloge on Priestley,* "L'Académie de Paris lui accorda un prix non moins noble et plus difficile encore à obtenir, parce qu'il est plus rare, l'une de ces huit places d'associés étrangers, auxquelles tous les savans de l'Europe

concourent, et dont la liste, commençant par les noms de Newton, de Leibnitz, et de Pierre le Grand, n'a dégénéré dans aucun temps de ce premier éclat." If any circumstance could enhance the value of such a distinction, it would be that he was the successor of Davy.

Mr. Dalton was present at the first meeting of the British Association held in York in 1831, and continued to feel a lively interest in its prosperity, and to attend the annual meetings as long as his health permitted him. On the occasion of the second meeting, at Oxford in 1832, the honorary degree of D.C.L. was conferred upon him, Mr. Faraday, Mr. Robert Brown, and Sir David Brewster. Rooms were provided for him, as a north countryman, in Queen's College; and the most courteous attentions were bestowed upon him and his companions by Mr. Wilson, the Senior Tutor, and the other resident Fellows of that Society. Dalton continued to wear in Oxford the doctor's red gown; and Prof. Sedgwick informs me, "Some one, I forget who, quizzed him about his scarlet covering, while we were before Magdalen College. 'You call it scarlet,' said Dalton; 'to me its colour is that of nature—the colour of those green leaves,' pointing to the trees." It was perhaps not one of the least services rendered to science by the first meetings of the British Association, that they brought before the notice of his countrymen the merits of Dalton, as being "one who might have had more of the fame had he been less satisfied with the possession of knowledge." I gather from letters in my possession, that Dr. Daubeny, the accomplished Professor of Chemistry and Botany in the University of Oxford, who has lately given another beautiful proof of his reverence for the memory of Dalton,* was the person who brought Dalton's

* In the following dedication of his Introduction to the *Atomic Theory*. "To the memory of John Dalton, the framer of a theory with respect to the mode of combination between bodies, which stands foremost among the discoveries of the present age for the universality of its applications and the importance of its practical results, holding the same kind of relation to the science of chemistry which the Newtonian system does to that of mechanics; and throwing light, not only upon all the ordinary subjects of chemical investigation, but even upon those more speculative questions, with respect to the constitution of matter, which seemed to lie beyond the reach of experi-

title, and that of his illustrious companions, to such a mark of distinction, before the governing body in Oxford.

In the summer of 1833 Lord Grey's Government conferred upon Dr. Dalton an annual pension of £150. I have found among my father's papers many letters and documents detailing all the circumstances that led to this act of royal favour. From these it appears that to Mr. Babbage unquestionably belongs the honour of having first suggested, in a letter addressed to my father as early as 1829, Dalton's title to such grant, and of having subsequently laboured most zealously, in conjunction with Mr. G. W. Wood, then M.P. for South Lancashire, to obtain the acquiescence of Government.* This was a task of great difficulty at a period when there was a violent popular outcry against pensions of all descriptions. Thus Mr. J. A. Murray (now Lord Murray) writes, "There is at present a dislike to pensions approaching to fanaticism. It is the cry of the times, arising no doubt from former abuses, and while it subsists to such a degree it is hardly possible to obtain for the most deserving what the nation ought to feel a pride in bestowing." The annexed statement was therefore prepared by my father, in

mental inquiry; this essay, which in a less mature form was honoured by his approval, is now inscribed, as a slight tribute to his posthumous reputation, by his former friend, and devoted admirer, The Author."

* Extract from a letter of the late Dr. Henry to J. A. Murray, Esq., M.P., February 26, 1833:—

"About four years ago an inquiry was put to me by a friend (Mr. Babbage) which I judged to be the forerunner of some act of Government in favour of Dr. Dalton, the venerable President of our Literary and Philosophical Society. Since that time several letters have passed between Mr. Babbage and myself on the subject; but, notwithstanding his zeal in favour of Dr. Dalton's claims, nothing has yet been done. Under these circumstances, it appears to me exceedingly desirable, by the vigorous and united efforts of all who approve the measure, to anticipate the time, which cannot be far distant, when this illustrious man (now I believe in his sixty-sixth or sixty-seventh year) will be beyond the reach of sublunary reward. Our county member, Mr. G. W. Wood, has kindly agreed to devote his best efforts to furthering the cause. We fully appreciate the difficulty of obtaining the grant of a pension (now a word of fearful omen especially to the grantors), but we are yet encouraged to hope for success by the soundness of Dr. Dalton's claims, and the little probability, from his rare excellence as a philosopher, that demands of equal weight can be made in favour of any other individual."

the form of a reply to an inquiry from Mr. Babbage "whether Dr. Dalton had ever received any reward or encouragement from Government," and was submitted, together with a formal application from the pen of Mr. Babbage,* to Lord Grey, Lord Brougham, Mr. Poulett Thomson, and other influential persons.

"Mr. Dalton never had, nor was ever given to expect, any reward or encouragement whatsoever from government; and having been in habits of unreserved communication with him for more than thirty years, I can safely aver that it never occurred to him to seek it. He has looked for his reward to purer and nobler sources,—to a love of science for its own sake; to the tranquil enjoyments derived from the exercise of his faculties, in the way most congenial to his tastes and habits, and to the occasional gleams of more lively pleasure, which have broken in upon his mind, when led to the discovery of new facts, or the deduction of important general laws. By the moderation of his wants, and the habitual control over his desires, he has been preserved from worldly disappointments; and by the calmness of his temper, and the liberality of his views, he has escaped those irritations that too often beset men, who are over anxious for the possession of fame, and are impatient to grasp prematurely the benefits of its award. For a long series of years he bore neglect, and sometimes even contumely, with the dignity of a philosopher who, though free from anything like vanity or arrogance, yet knows his own strength; estimates correctly his own achievements, and leaves to the world, generally though sometimes slowly just, the final adjudication of his fame. Among the numerous honours that have since been conferred upon him, by the best judges of scientific merit in this and other countries, not one has been sought by him. They have been, without exception, spontaneous offerings, prompted by a warm and generous

* Respecting this document, Mr. Wood writes, "Mr. Babbage has executed his task very much to Mr. P. Thomson's and my own satisfaction, and the paper is in Mr. Thomson's hands, and I hope he will now soon speak with Lord Grey on the subject."

approbation of his philosophical labours, and by the desire to cheer him onward in the same prosperous career. Deeply as he has felt these distinctions, they have never carried him beyond that sober and well regulated love of reputation, which exists in the purest minds, and is one of the noblest principles of action.

"In perfect consistency with Mr. Dalton's intellectual qualities, are the moral features of his character; the disinterestedness, the independence, the truthfulness, and the integrity which, through life, have uniformly marked his conduct towards others. He has been taxed with plagiarism, but never was a charge more completely unfounded. Not only is he incapable of encroaching upon the just rights of others, but even of taking *tacitly* to himself applause to which he does not feel that he is fully entitled. Of the work, from which he is accused of having borrowed the outline of the atomic theory, he had never even heard, until many years after the publication of his opinions on that subject. Nor is this at all extraordinary, when it is considered that men, like Mr. Dalton, of original and creative minds, trust rather to their own powers of research, than to reading; and, in the knowledge of the history of science, are often surpassed by very inferior persons. This general remark applies to Mr. Dalton: but he is a *discoverer* in the true sense of the word. He has drawn from observed phenomena, new and ingenious views; upon these views he has founded distinct conceptions of a general law of nature; and he has traced out the conformity of that law with an extensive class of facts, many of which he himself first revealed by well-devised experiments. He has thus secured our admiration, not by having broached ingenious opinions merely, but by having worked out the evidences of these opinions by labour most sagaciously and perseveringly applied. Nor is it on the atomic theory only that his reputation must rest. It has a broader basis in his beautiful and successful investigations into the subject of heat;—into the relations of air and moisture to each other,—and into a

variety of other topics, intimately connected with the stability and advancement of chemical philosophy.

"I therefore agree with you, that Mr. Dalton has strong claims upon the national respect and gratitude, and contend for his title to reward, even though he may not have accomplished anything bearing *directly* upon the improvement of those arts and manufactures, which are the chief sources of our national wealth. For let it be remembered that every new truth in *science* has a natural and necessary tendency to furnish (if not immediately, yet at some future time,) valuable rules in *art*. Nothing is more common than that a general principle, when first developed, may admit of no obvious practical use; but that a few subsequent discoveries, made perhaps at a small expense of genius or labour, supply links, which render it available, first to individual, and, in due course, to public wealth and prosperity. Not to mention other instances, Mr. Watt derived from Dr. Black's discovery of latent heat a guiding light to the noblest invention that has ever been placed in the hands of man, for giving him control over the physical world, and even for advancing his progress in moral and intellectual cultivation. The discovery of chlorine also, in the laboratory of a retired chemist, brought forth no practical benefits for several years; but when found, by a subsequent philosopher, to quicken the whitening of unbleached cotton and linen goods, it was immediately applied by practical men to the art of bleaching; and no one can now calculate its immense influence in giving rapid circulation to the capital employed in the cotton and linen manufactures. Among the abstract truths unfolded by Mr. Dalton, it would not be difficult to point out the germs of future improvements in the practical arts,—germs which now lie dormant in the shape of purely scientific propositions.

"But were it otherwise, it would surely be unworthy of a great nation, to be governed in rewarding or encouraging genius, by the narrow principle of a strict barter of advantages. With respect to great poets and great historians, no such parsimony has ever been exercised. They have been

rewarded (and justly) for the contributions which they have cast into the treasury of our *purely intellectual* wealth. And do not justice and policy equally demand, that a philosopher of the very highest rank,—one who has limited his worldly views to little more than the supply of his natural wants, and has devoted, for more than forty years the energies of his powerful mind to enlarging the dominions of science,—should be cherished and honoured by that country, which receives by reflection the lustre of his well-earned fame? The most rigid advocate of retrenchment and economy cannot surely object to the moderate provision, which shall exempt such a man, in his old age, from the irksome drudgery of elementary teaching, and shall give him leisure to devote his yet vigorous faculties to reviewing, correcting, applying and extending, what he has already, in great part, accomplished. In one instance of recent date, a philosopher, who has eminently distinguished himself in purely abstract science, has received the merited reward of a pension for life. It is most desirable then that the British government, by extending its justice to another not less illustrious, should be spared the deep reproach which otherwise assuredly awaits it,—of having treated, with coldness and neglect, one who has contributed so much to raise his country high among intellectual nations, and to exalt the philosophical glory of the age."

Mr. Murray, brought this letter before the notice of the Lord Chancellor, "who said as I expected, that no person felt more strongly than he did, Dr. Dalton's merits, and that he was very anxious to obtain some provision for him, but that it was attended with great difficulty."* These difficulties were finally surmounted by the strenuous exertions of Mr. Poulett Thomson and Mr. G. W. Wood.

Dr. George Wilson, in an admirable article on the life and writings of Dalton, in the British Quarterly Review, has stated that Dr. Chalmers was mainly instrumental in obtaining the pension for Dalton, by influencing Mr. Hume in its favour. I do not doubt, that Dr. Chalmers, who knew and respected Dalton, did write to Mr. Hume on the subject;

* Letter from J. A. Murray, Esq.

but I do not find any indications in the numerous letters in my possession, from Mr. G. W. Wood to my father, that Mr. Hume took any active part in the matter. It seems rather, from the following passages, that all that was asked for from "economy's great apostle," as Mr. Wood designates him, was passive acquiescence. "Mr. Babbage thinks it very important, to secure or neutralize, what is termed the *mountain* party in the House. Mr. Warburton is the proper channel for this on a scientific question. Would you like to write to him on the subject? or if you would prefer my calling on him for the purpose, I would do it with pleasure, if you would favour me with a note to him." Again, "In all matters of money, it is considered desirable to propitiate Mr. Hume, and to secure his approval or forbearance beforehand." Before concluding this narrative, I feel anxious to place on record, the following extract from a letter to myself, from Mr. Wood, dated Singleton Lodge, June 23, 1833. "I am now released from secrecy, and lose no time in acquainting you with the fact, and sincerely congratulate you on the success of your father's honourable, judicious, and persevering exertions, for the attainment of an object so desirable for our friend, and at the same time so honourable to the government and creditable to the country."

It was thought desirable that the announcement should first be made to Dalton at the approaching meeting of the British Association at Cambridge; and Lord Monteagle, just before the meeting, communicated the fact of the grant to Professor Sedgwick, the President of the Association, with a request that he would allude to it in his introductory address. I am greatly indebted to Professor Sedgwick for a copy of the words, in which, without notes or preparation, he offered his fervent heartfelt homage to the genius of Dalton.

"They had all read a highly poetical passage of a sacred prophet, expressed in language, to the beauty of which he had never before been so forcibly awakened as at that moment:—'How beautiful upon the mountains are the feet of him that bringeth good tidings.' If he might dare to

make an adaptation of words so sacred, he would say, that he felt himself in the position here contemplated,—of one who had the delightful privilege of announcing good tidings, for it was his happiness to proclaim to them what would rejoice the heart of every true lover of science. There was a philosopher sitting among them, whose hair was blanched by time, whose features had some of the lines of approaching old age, but possessing an intellect still in its healthiest vigour,—a man whose whole life had been devoted to the cause of truth; he meant his valuable friend, Dr. Dalton. Without any powerful apparatus for making philosophical experiments,—with an apparatus, indeed, many of them might almost think contemptible,—and with very limited external means for employing his great natural powers, he had gone straightforward in his distinguished course, and obtained for himself in those branches of knowledge which he had cultivated, a name not perhaps equalled by that of any other living philosopher of the world. From the hour he came from his mother's womb, the God of Nature had laid his hand upon his head, and had ordained him for the ministration of high philosophy. But his natural talents, great as they were, and his almost intuitive skill in tracing the relations of material phenomena, would have been of comparatively little value to himself and to society, had there not been superadded to them a beautiful moral simplicity and singleness of heart, which made him go on steadily in the way he saw before him, without turning to the right hand or to the left, and taught him to do homage to no authority before that of truth. Fixing his eye on the highest views of science, his experiments had never an insulated character, but were always made as contributions towards some important end,—were among the steps towards some lofty generalization. And with a most happy prescience of the points towards which the rays of scattered experiments were converging, he had more than once seen light, while to other eyes all was yet in darkness;—out of seeming confusion had elicited order, and had thus reached the high distinction of becoming one of the greatest legislators of chemical science.

"While travelling among the highest mountains of Cumberland, and scarifying the face of Nature with his hammer, he (the President) had first the happiness of being admitted to the friendship of this great and good man, who was at that time employed, day by day, in soaring among the heavens, and bringing the turbulent elements themselves under his intellectual domination. He would not have dwelt so long on these topics, had it not been his delightful privilege to announce for the first time (on the authority of a minister of the crown who sat near him), that His Majesty King William the Fourth, wishing to manifest his attachment to science, and his regard for a character like that of Dalton, had graciously conferred on him, out of the funds of the civil list, a substantial mark of his royal favour." The announcement was received with long continued applause.

Mr. Wood, who was present, wrote to my father:— "Dr. Dalton received the intelligence very nicely, and with his customary quietness and simplicity of manner; but he was a good deal affected when Professor Sedgwick pronounced his eloquent eulogium on him in the senate house."

The formal announcement was made to Dalton by Mr. Poulett Thomson, in the following letter:—

"Whitehall, June 22, 1833.

"Sir,—Although I have not the honour of enjoying your personal acquaintance, the gratitude which I feel for the distinguished services you have conferred upon science, as well as the respect which I entertain for your character, made me feel deeply anxious that some public mark of those feelings which are, I believe, common to the country, should be shown by the Government to which I belong, and induced me earnestly to press for the first opportunity being taken to offer such a testimony, however trifling the pecuniary amount of it might be. It is therefore with sincere pleasure that I inform you that His Majesty has been graciously pleased to second their wishes in the manner, which you will perceive from the accompanying note from Colonel Grey, which I have the honour to enclose.

"I beg to subscribe myself, with the most sincere respect, Sir, "Your faithful servant,

"C. POULETT THOMSON."

The note enclosed was:—

"DEAR THOMSON,—My father desires me to tell you that the King has been pleased to grant a pension of £150 nett to Mr. Dalton. "Yours very truly,

"C. GREY."

After the lapse of three years, Mr. Thomson wrote to inform Dalton that the amount of the pension had been doubled.

"June 8, 1836.

"MY DEAR SIR,—I have for some time felt that there might be some ground for complaint on the part of the friends both of science generally and of yourself, in the circumstance of the pension, which was conferred upon you in the year 1833, being inferior in amount to those which have since been allotted to other distinguished scientific persons, and the inquiries which I have been enabled to make at Manchester, have led me to think that if I could succeed in correcting this inequality, it might be the means not only of proving to the world the high esteem in which your great labours are held, but of enabling you to devote more time to your important studies,—a part of which I have learnt is still dedicated to elementary instruction.

"It is with this view that I have (although entirely unknown to yourself) mentioned the subject to Lord Melbourne; and I am happy to have it in my power to state to you, that his Lordship has accordingly given directions that a pension of £150 should be added to that already held by you, making in all £300.

"I feel great pleasure in making this communication to you, and I trust that it will not be less agreeable to yourself than I am satisfied it will be to all the friends of science, and to those who are personally your friends and admirers, amongst whom, I hope I may be allowed to range myself.

"Believe me, my dear Sir,

"Yours very faithfully,

"Dr. Dalton." "C. POULETT THOMSON.

While these efforts were being made, which resulted in Dalton's receiving a mark of royal bounty, there was a simultaneous movement in progress among his friends in Manchester, to raise, by subscription, a sum to be employed in some local demonstration in his honour. The precise form which this should assume, was not at first defined; and various projects, among others, that of erecting an institution for scientific purposes, were entertained and finally abandoned for that of a portrait statue. I venture to extract the following passage from a letter from my father to the late Mr. Ewart: "I trust that the Committee will pause before they determine to abandon the proposition of a statue, and will decide on handing down to distant posterity, the *viva ac vera effigies* of a man who will be honoured in all future ages, so long as science shall be known and respected. It will be a bequest, which future philosophers, as well as the world at large, will know how to appreciate. It will gratify the desire inherent in all men, to call up distinct conceptions of the visible form and features, which have been associated with intellectual endowments of the highest order. How much such resemblances are prized will appear from the following extract of a letter recently addressed by Berzelius (one of the first of living philosophers) to a friend, (Dr. Traill), who had sent him the portrait of Dr. Dalton. "Mille et mille remercîmens pour ce cadeau. Je suis bien aise d'avoir une idée de la figure d'un homme, dont une pensée heureuse a été si fertile en résultats scientifiques." I find the following sketch of a report for the Dalton Committee, by the same pen, which was sent to Dr. Bardsley and Dr. Calvert.

"February 28, 1834.

"The Committee appointed to take measures for obtaining a statue of Dr. Dalton have great pleasure in reporting that the object is now in a fair way to be accomplished. Finding the proposal to be warmly seconded by the general view of the inhabitants of Manchester and the surrounding districts, and to be supported by willing and liberal contributors,* it

* Spontaneous offerings flowed in also from distant friends and admirers of Dalton. Foremost among them was a liberal contribution from Dr. Faraday, which he did me the honour of transmitting through me.

appeared to them to be time to seek such information as might enable them to decide to what sculptor, a work destined, it is hoped, to last for ages, should be confided. With the highest respect for other artists, whose names have been mentioned, some of them established in celebrity, and others fast rising into reputation, the Committee have fixed their view on Mr. Chantrey, as one eminently distinguished, not only in the inventive province of his art, but by the fidelity with which, in more than one recent instance, he has pourtrayed the lineaments of men grown old in intellectual pursuits, and by the success with which he has caught that expression of calm and patient thought, and that capacity for lofty contemplation, which nature and habit had imprinted on their features.

"To the inquiry addressed to Mr. Chantrey for information respecting his usual terms, that gentleman has replied in a liberal spirit, not insisting on those terms to their full extent, and declaring that the leading incitement with him to execute the work, to the best of his ability, will be his heartfelt respect for the subject of the memorial, and his anxiety to be remembered by posterity in connection with so great a name.

"The Committee, by a deputation consisting of its chairman, and a few other members, communicated to Dr. Dalton their wish that he should afford to Mr. Chantrey the necessary facilities, at the earliest period, that will suit their mutual convenience; and in this request he acquiesced with the modesty, simplicity, and excellent feeling that grace his character. There is, therefore, every reason to hope, that at a period not much exceeding two years, we shall possess and hand down to posterity, protected from decay or injury, within the walls of the Royal Institution into which the governors have cordially agreed to receive it, a noble work of art, not only a memorial of our high respect for Dr. Dalton, and of the pride we take in him as a fellow-townsman, but a proof that *we* in *our* generation, were not incapable of estimating the genius of the philosopher, nor slow in paying homage to the virtues of the sage."

In May 1834, I had the pleasure of accompanying Dr. Dalton to London for this object, and of being present at several of his sittings to Chantrey. There were many elements of character and outward manner common to the great sculptor and philosopher. They were each alike intent upon grand objects, alike ardent worshippers of truth, servants and interpreters of nature, and were equally indifferent to external forms and conventions. Chantrey's genial straightforward manners placed Dalton at once at his ease; and both being north-countrymen, fresh and vigorous natures, they were at no loss for topics of conversation. But we possess Dalton's own impression of these interviews in a letter to the late Mr. Peter Clare. May 2nd, 1834.

"Next morning Mrs. Wood walked through the park with me to Mr. Chantrey's; when we found him in expectation of seeing me. He took a profile as large as life by a camera lucida, and then sketched a front view of the face on paper. We took a walk through his rooms, and saw busts and statues without end. He then gave me the next day for a holiday, and told me I should see my head moulded in clay on Wednesday morning, at which time he invited me to breakfast. I went accordingly, and found, as he said, a head *apparently* perfect. He said he had not yet touched it, the head having been formed from his drawings by some of his assistants. He set to work to model and polish a little whilst I was mostly engaged in reading the newspaper, or conversing with him. On looking right and left he found my ears were not alike, and the modeller had made them alike, so that he immediately cut off the left ear of the bust and made a new one more resembling the original. Most of the time I was amusing myself with viewing the pictures and statues in the room. At last he took a pitcher and blew a little water in my face (I mean the model), and covered my head with a wet cloth and we parted, he having desired me to bring Dr. Henry and Dr. Philp with me next morning to breakfast. We went accordingly, and found an abundant table; soon after Dr. Faraday came in, and we all went into the working room for a time. This morning, (sixth day)

Mrs. Wood was kind enough to walk with me again to Mr. Chantrey's, and we spent another hour or two under his directions. At intervals we have a little amusement and instruction about our respective arts and sciences, and how we acquired our knowledge, &c., in which we vie with each other, and keep up a lively conversation."

An engraving from Chantrey's bust is prefixed as a frontispiece to this volume. From this bust Sir Francis Chantrey afterwards modelled the statue of Dalton, of the size of life, now preserved in the entrance hall of the Manchester Royal Institution, and one of the most impressive and intellectual portrait statues by that great artist. A more refined and ideal expression has been bestowed upon the countenance in the statue. The bust is the more faithful portraiture of the philosopher. It was on the occasion of this stay in London that he was first formally admitted, as Fellow of the Royal Society, being presented by Mr. Davies Gilbert to Sir Benjamin Brodie, President for the evening. He was also presented at court through the instrumentality of Mr. Babbage, and appeared in the scarlet gown of an Oxford Doctor of Civil Law.

In the autumn of this year, on the occasion of the meeting of the British Association at Edinburgh, the degree of LL.D. was conferred upon him by the unanimous vote of the Senate of the University. Dr. Traill writes to him: "We were quite aware that Oxford had done itself honour by conferring a similar degree on you; but we were also anxious to testify our respect and admiration for the scientific attainments of the author of the atomic theory. As such please to accept it."

Mr. Babbage has kindly sent me the following amusing narrative of Dalton's presentation at Court:—

"My dear Sir,—I have now examined, as far as I can, my papers, to find any traces of Dalton amongst them. I can find only two letters, of which I send you copies.

"I well remember taking a great interest in Dalton's pension, as you will see by several passages in *The Decline of Science*, pp. 20 and 22, and note; but I have no recollection

of any of the circumstances, or through what channel it was applied for.

"I find several letters of that date from Mr. Wood, and it appears from them that I went with him to Poulett Thomson; but I only gather this fact from those letters. I send them in the inclosure, as they may be of use. You can return them at your own convenience.

"When the inhabitants of Manchester had subscribed 2,000*l.* for a statue of Dalton, he came up to London, and was the guest of Mr. Wood. He sat to Chantrey for the statue. I consequently saw much of my friend. It occurred to me that, as his townsmen were having a statue of him—as the University of Oxford had given him the honorary degree of Doctor of Laws—and as the Government had given him a pension—if it were not incompatible with his feelings, it would be a fit thing that he should be presented at a levee. It appeared to me that if William the Fourth were informed of it, it would afford him an opportunity of saying a few words to the venerable philosopher, which would be gratifying to the inhabitants of Manchester, the University of Oxford, and the world of science.

"Accordingly I wrote a note to Mr. Wood suggesting the idea, and proposing that he should ascertain from Doctor Dalton whether it would be unpleasant to him to go through the forms.

"Dalton not objecting, my note was sent on by Mr. Wood to Lord Brougham, who at that time was Lord Chancellor. He approved highly of the plan, and offered to present Doctor Dalton. He also mentioned the circumstance to the King.

"I had had some conversation with Mr. Wood about the subject, when several difficulties presented themselves to him. Doctor Dalton, as a quaker, could not go in a court dress, because he must wear a sword. To this I replied, that being aware of this, I had proposed to let him wear the robes of a Doctor of Laws of Oxford.

"Mr. Wood remarked, that those robes being *scarlet*, they were not of a colour admissible by Quakers.

"To this I replied, that Doctor Dalton had a kind of *colour blindness,* and that all red colours appeared to him to be the colour of dirt. Besides, I had found that our friend entertained very reasonable views of such mere matters of form. The velvet cap of the Doctor again was not an obstacle, as he was informed that it was usually held in the hand, and was rather a mark of office, than a covering for the head.

"These difficulties being surmounted, Doctor Dalton came one morning to breakfast with me. We were alone; and after breakfast he went up with me into the drawing-room, in order to see the Difference Engine. After we had made several series of calculations, he recollected that he had in his pocket a note from Mr. Wood to me. On hastily looking over it, I found that it was to announce to me that our friend acquiesced in the scheme.

"I now mentioned the forms usual at a levee, and placing several chairs in order to represent the various officers in the presence chamber I put Doctor Dalton in the middle of the circle to represent the King. I then told my friend that I should represent a greater man than the King; that I intended to personate Doctor Dalton, and would re-enter at the further door, go round the circle, make my obeisance to the King, and thus show him the kind of ceremony at which he was to assist.

"On passing the third chair from the King's, I put my card on the chair, at the same time informing Doctor Dalton that this was the post of a Lord in Waiting, who takes the cards, and gives them to the next officer, who announces them to the King.

"On passing the philosopher I kissed his hand, and then passing round the rest of the circle of chairs, I thus gave him his first lesson as a courtier.

"It was arranged that I should take Doctor Dalton with me to the levee, and put in his card, 'Doctor Dalton, presented by the Lord Chancellor.'

"When the morning arrived I went to Mr. Wood's residence, and found Doctor Dalton quite ready for the expedi-

tion. In order to render the chief actor perfect in his part, we again had a rehearsal; Mrs. Wood personating the King, and the rest of the family, with the assistance of sundry chairs and stools, the great Officers of State. I then entered the room, preceding my excellent friend, who followed his instructions as perfectly as if he had been repeating an experiment.

"Being now quite satisfied with the performance, we drove off to St. James's. The dress of a Doctor of Laws is rarely made use of, except at a University address, and Doctor Dalton's costume attracted much attention, and compelled me to gratify the curiosity of many of my friends, by explaining who he was. The prevailing opinion was that he was the Mayor of some corporate town come up to get knighted. I informed my inquirers, that he was a much more eminent person than any Mayor of any city, and having won for himself a name which would survive when orders of knighthood should be forgotten, he had no ambition to be knighted.

"At a short distance from the presence chamber, I observed close before me several dignitaries of the Church, in the full radiance of their vast lawn sleeves. The Bishop of Gloucester, who was nearest to me, accidentally turning his head, I recognized a face long familiar to me from its cordiality and kindness. A few words were interchanged between us, and also by myself with the rest of the party, the remotest of whom, if I remember rightly, was the Archbishop of Dublin. The dress of my friend seemed to strike the Bishop's attention; but the quiet costume of the Quaker beneath his scarlet robe was entirely unnoticed. I therefore confided to the Bishop of Gloucester the fact that I had a quaker by my side, at the same time assuring him that my peaceful and philosophic friend, was very far from meditating any injury to the Church. The effect was electric upon the whole party; episcopal eyes had never yet beheld such a spectacle in such society, and I fear, notwithstanding my assurance, some portion of the establishment thought the Church really in danger.

"We now entered the presence chamber, and having passed the King, I retired very slowly, in order that I might observe events. Doctor Dalton having kissed hands, the King asked him several questions, all which the philosopher duly answered, and then moved on in proper order to join me. This reception, however, had not passed with sufficient rapidity to escape jealousy, for I heard one officer say to another, 'Who the D—l is that fellow whom the King keeps talking to so long?'

"Conversations at Courts are not always thought to be the most interesting things in the world; although, doubtless, they must be so to the parties engaged in them. In the midst of crowded levees and drawing-rooms, one is often compelled to become the confidant of strangers around us. The amusement derived from this source predominates over the instruction. I have heard much anxious inquiry as to certain pieces of clerical preferment—who is to have certain military or colonial commands, and what promotions will take place from the consequent vacancies?—many political queries have been proposed, and how 'the party' would act in certain contingent cases? I once heard a gentleman receive at a levee the first announcement of a legacy; on another occasion, on my return from the continent, I was myself informed at a levee of a similarly gratifying, and to me entirely unexpected event.

"Doctor Dalton having now passed through the formal part of a levee had a better opportunity of viewing the details. He inquired the names of several of the portraits, and I took the opportunity of pointing out to him many of the living celebrities.

"We then returned to Mr. Wood's residence, and the whole party seemed gratified at the success of the undertaking.

"I am, my dear Sir, very truly yours,

"C. BABBAGE.

"*Dorset Street, Manchester Square,*

"*February* 7, 1854."

CHAPTER VIII.

HIS BROTHER'S DEATH — DUBLIN AND BRISTOL MEETINGS — FIRST ATTACK OF PARALYSIS, 1837—LORD BURLINGTON'S ADDRESS TO THE LIVERPOOL MEETING —PAPERS ON THE PHOSPHATES AND ARSENIATES, ON MICROCOSMIC SALT, MIXTURE OF SULPHATE OF MAGNESIA AND BIPHOSPHATE OF SODA, WATER OF CRYSTALLIZATION AND ANALYSIS OF SUGAR—IMPORTANT DISCOVERY CONFIRMED BY MM. PLAYFAIR AND JOULE—DR. PLAYFAIR'S SUMMARY OF THEIR MAIN RESULTS—MEETING OF BRITISH ASSOCIATION IN MANCHESTER, 1842—LORD FRANCIS EGERTON'S NOTICE OF DALTON—HIS DEATH, JULY 27, 1844 —MR. RANSOME'S ACCOUNT OF HIS ILLNESS — PUBLIC FUNERAL — WILL — FOUNDATION OF SCHOLARSHIPS AND BRONZE STATUE.

In December 1834 Mr. Jonathan Dalton died, leaving all his real and personal estate to his brother. It is worthy of remark that both brothers died of the same disease—paralysis, and that both survived the first seizure some years. John writes thus to a distant relative: "My brother died, after having been incapacitated for business for four or five years by a gradually induced paralysis. He was capable of walking two or three hundred yards with a stick till a year before his death, when he walked by the assistance of a man or was drawn along in a chair. His mental faculties were preserved to the last."

Dr. Dalton was present at the meetings of the British Association in 1835, at Dublin; and in 1836, at Bristol, and officiated on both occasions as Vice-President of the Chemical Section. He did not communicate any paper to either meeting. He had preserved a copy of a characteristic note to Mr. Exley, whose acquaintance he had formed at Bristol.

"I return you my thanks for your *Principles of Natural Philosophy* which you have presented to me. I have looked

over it, and seen several things to admire, but I am not yet become a convert to your doctrine. We agree about atmospherules of heat without sensible weight; but the central or gravitating atoms you seem to think may be either something or nothing, whereas I think they must be something."

It was on the 18th of April, 1837, that he had the first attack of paralysis, which was followed by a second and slighter seizure three days afterwards. The last entries in his letter-book, in his firm distinct hand, are copies of testimonials to Mr. Graham and Dr. Clark, who were then candidates for the Chair of Chemistry in the London University, and are dated April 12th and 13th. After these there occurs an interval, till August 17th; and his handwriting becomes increasingly tremulous and indistinct, and his letters briefer till August 1842, when he apparently ceased to write, or at least to preserve copies of his letters. Early in June 1837 he had sufficiently recovered to be able to send to the Royal Society his memoir, entitled "Sequel to an Essay on the Constitution of the Atmosphere," published in the *Philosophical Transactions for* 1837, and already noticed. I possess however the rough copy, which is written in his usual hand, and had been entirely composed before his illness. There are no indications of its having been subsequently altered. In September of the same year the British Association assembled at Liverpool, and Dr. Dalton had been chosen one of the Vice-Presidents. He was unable to be present, for the first time since the formation of the Association. The Earl of Burlington, as President, observed, "The venerable Dr. Dalton was also absent: the infirmities of increasing age had compelled him to abstain from meeting those who delighted to honour the philosopher, whose life had been devoted to science, and whose reward had come late; but it was a reward whose justice all acknowledged, and the honours conferred on Dr. Dalton were as gratifying to the public as to himself." He, however, communicated through a friend a short paper "On the Non-Decomposition of Carbonic Acid by Plants." "He calculates that in five

thousand years animals supposed to live upon the earth would produce but ·001 of carbonic acid, so that the assistance of plants to purify our atmosphere is not necessary. By experiment he found that a hot-house does not contain more or less carbonic acid, by night or by day, than the external air, and the results were the same in a number of repetitions of the experiments. This paper was said to have been penned during the convalescence of its illustrious author from a late attack of illness, and was listened to with the greatest attention."* The origin of these experiments will be found hereafter detailed in Mr. Ransome's letter.

In the year 1840, Dr. Dalton communicated to the Royal Society an Essay on the Phosphates and Arseniates. Its main purport is to reassert his old ideas † that the equivalents of the tribasic phosphates, of phosphoric acid, and of phosphorus itself should be taken at one-third of the received numbers; for which he adduces the new argument, that the water of the subphosphate of soda, which Professor Graham had found to be twenty-four equivalents, is divisible by three and gives eight equivalents. This he is disposed to believe is the highest number of equivalents of water which a salt ever possesses. But how entirely inadmissible this division of phosphoric acid by three is, may be judged from the circumstance that it would require that acid to contain six instead of five equivalents of oxygen. This paper is throughout obscure, and in parts scarcely intelligible. It is the obvious product of a mind seriously weakened by disease. There can be no question that the Council of the Royal Society in declining to publish it, were governed by a true regard to Dalton's lasting reputation. He himself was much mortified by their decision; and, having procured a copy of his essay from the archives of the Society, printed it in a separate form, with the indignant comment, "Cavendish,

* *The Athenæum*, No. 519, p. 748.

† In a previous letter to Mr. Johnston, dated February 1836, he thus speaks of Berzelius's view of the compounds of phosphorus and oxygen: "Nature never made such a series of combinations, and she defies art to produce a more tortuous set."

Davy, Wollaston, and Gilbert are no more." He concludes, "I intend to print my essays in future, to be appended to my other publications. Some of them are materially affecting the atomic system."

These short essays, four in number, viz. "On Microcosmic Salt;" "On the mixture of Sulphate of Magnesia and the Biphosphate of Soda;" "On the Quantity of Acids, Bases, and Water in the different Varieties of Salts, with a new method of measuring the water of crystallization, as well as the acids and bases; and "On a new and easy Method of Analysing Sugar," were soon afterwards printed by him collectively. The last two announce a discovery of considerable importance. He found that certain salts, rendered perfectly anhydrous by heat, when dissolved in water caused no increase of its volume, "showing that the salt enters into the pores of the water;" also that salts containing water, when dissolved in a measured quantity of pure water, increased the volume of the solvent by a quantity precisely equal to their constituent water, the solid matter, as before, "entering the pores of the water." Sulphate of magnesia was the subject of several experiments; but he adds, "I have tried the carbonates, the sulphates, the nitrates, the muriates or chlorides, the phosphates, the arseniates, the oxalates, the citrates, the tartrates, the acetates, &c., &c., and have been uniformly successful: only the water adds to the *bulk*, and the solid matter adds to the *weight*." "This fact," he continues, in his last paper on sugar, "was new to me, and I suppose to others. It is the greatest *discovery* that I know of next to the atomic theory." He proceeds to apply this new principle to the analysis of sugar. "The experimentum crucis after all is in the following example. Take 100 grains of sugar, dissolve it in 100 water, which will just melt it; then pour it out into a glass measure of upwards 160 grains; it will be found 157 grains precisely. The 57 grains of pure water have arisen out of the sugar, and the 43 grains remain in, buried invisibly in the pores of the water." Now Gay-Lussac and Thénard had determined the composition of sugar by combustion as—

Charcoal	42·47	and Dr. Prout 42·85
Water .	57·53	57·15
	100·0	100·0

results which accord most precisely with that of Dalton.

These facts were regarded as highly improbable by those to whom they were first announced; but they have been subsequently confirmed, at least as respects cane-sugar and certain salts, by the elaborate researches of Dr. Lyon Playfair and Mr. Joule.

I am indebted to the kindness of Dr. Lyon Playfair for the following interesting summary of the main results which bear on Dalton's views:—

"Messrs. Playfair and Joule have published several memoirs on the subject of atomic volumes, in which they have given numerous specific gravities, and have deduced certain general results, which may be regarded as well established; while others, brought forward by them as probable, are still open to new experiments, and require confirmation.

"In the first place, they verified Dalton's assertion to the extent, that the volumes of highly hydrated salts in solution are equal to those of their equivalents of water, the volume of the salt itself disappearing. Thus, 1 eq. of NaO,CO_2+10HO taken in grains, as representing its equivalent, and dissolved in water, increases the bulk of the latter exactly as if ninety grains of water had been added to the original water employed for solution. The phosphates and arseniates of soda act in a similar way, and cane sugar behaves in a like manner. So remarkably is this the case, that if anhydrous sulphate of copper be dissolved to a certain extent in water, the bulk of the latter, after solution, is actually *less* than it was before, the reason being, that a small but sensible condensation takes place from the anhydrous sulphate of copper attaching water to itself in chemical union during the act of solution.

"Playfair and Joule extended the views of Dalton, as regarded the liquid volume, to the volume of the salt in

the solid state. Proceeding on the supposition, that the volume of water in the solid salt acquires the volume of ice, or of the only solid form of water known, they established the fact, that the bulk of the highly hydrated salts, even in the crystallized state, consists of the bulk due to the equivalents of water viewed as ice. The specific gravity of ice being 0.9184, its atomic volume is $\frac{9}{0.9184} = 9.799$ or 9.8. The solid volumes of the carbonates, phosphates, and arseniates of soda are just equal to the ice-volume multiplied by the number of equivalents of water. This singular fact is shown in the following table:—

Salt.	Formula.	Equivalent.	Vol. of Salt by Experiment.	9·8 or Vol. of Ice as Unity.	Volume by Calculation.	Specific Gravity by Calculation.	Specific Gravity by Experiment.
Carbonate of soda	$NaO,CO_2 + 10HO$	143·4	98·6	10	98·0	1·463	1·454
Phosphate of soda	$2NaO,HO,PO_5 + 24HO$	359·1	235·5	24	235·2	1·527	1·525
Subphosphate of soda	$3NaO, PO_5 + 24HO$	381·6	235 2	24	235·2	1·622	1·622
Arseniate of soda	$2NaO,HO,AsO_5 + 24HO$	402·9	232·0	24	235·2	1·713	1·736
Subarseniate of soda	$3NaO,AsO_5 + 24HO$	425·2	235·6	24	235·2	1·808	1·804
Cane sugar	$C_{12}(H_{11}O_{11})$	171·	107·4	11	107·8	1·586	1·593
Milk sugar	$C_{24}(H_{24}O_{24})$	360·	234·7	24	235·2	1·531	1·534

"This was a remarkable confirmation of Dalton's views, and in a direction that he does not seem to have anticipated. It was the more satisfactory, because the specific volume of the solid may be obtained with much more precision than the volume of the same body when dissolved in water. In fact, in few cases has the liquid volume of salts in solution been determined with much accuracy. Every

solution, according to its strength, has a different rate of expansion by heat; and, therefore, when the volumes of solutions are compared at the same temperature, this is not a correct method of estimating them. But to examine the rate of expansion of solutions of any salt at different strengths, and to carry these experiments over a large range of substances, involves a task of so much labour, that no chemist has yet performed it with accuracy. But with the verification of the liquid volumes of the highly hydrated salts, as derived from the solid volumes of the same salts, no doubt now exists as to the accuracy of Dalton's views in regard to this limited class of salts.

"Dalton, however, was inclined to generalize the observation much further than it would bear. There is, indeed, a class of salts which occupy in solution only the volumes due to their water, although, in the solid state, they possess volumes due to one or more of their anhydrous constituents. Playfair and Joule have shown this to be true in the case of the magnesian sulphates, such as those of copper, zinc, magnesia, iron, and in a few other salts, as biborate and pyrophosphate of soda,—the liquid volumes in all these salts being due to their water of crystallization; while their solid volumes are the product of their equivalents of water *quasi* ice added to the volume of their base, the acid having disappeared or become merged in volume. But, again, there are numerous salts, such as the nitrates, chlorides, chromates, oxalates, &c., which, when anhydrous, or when only slightly hydrated, occupy specific volumes of their own, both in solution and in the crystallized state. The law regulating these is certainly not yet clearly made out. Playfair and Joule believed, that they had ascertained that there was a simple relation between the volume in solution and the volume in the solid state; but the difficulties of determining the former with precision render this relation still an open question.

"We may sum up the researches of Playfair and Joule in their bearing on Dalton's views as establishing the following facts:--

1. That the volumes of both the acid and base of highly hydrated salts become merged when they are dissolved in water.

   a. That the volumes of the acid and base of these salts are also merged in their crystallized state, the solid volume being that due to the water of crystallization viewed as ice.

2. That there is a certain class of salts, like the magnesian sulphates, which dissolve in water with a volume due to their water of crystallization alone; but that the volume of the latter is not sufficient to account for their bulk in the solid state.

   a. In these cases it is generally only the volume of the base which is added to the bulk of the water of crystallization in the solid state, the volume of the acid being merged.

"In a paper read at the British Association at Edinburgh, Playfair has given proofs of the accuracy of the latter law, by applying the formula flowing from it to the discussion of the volumes of many minerals. Chalcolite, Uranite, Kupferschaum, Vivianite, Wavellite, Peganite, Gibbsite, Atacamite, and other minerals, were shown to give volumes quite accordant with the laws here stated. In the cases of hydrated silicates, however, the condensation was remarkable, the tendency in them being for the silica to occupy volume along with the water, the bases frequently disappearing altogether in bulk."

In June, 1842, the British Association, for the first and only time, assembled in Manchester. Dr. Dalton's impaired articulation and infirm state of health rendered it obviously impossible for him to fill the office of president, which must otherwise, by universal consent, have devolved upon him. He was, therefore, appointed one of the vice-presidents; and Lord Francis Egerton, who occupied the chair, thus gracefully acknowledged Dalton's undoubted title to the higher distinction:—"Manchester has, in my opinion, a claim of equal interest as the birth-place, and still the residence and scene of the labours of one whose name is uttered with

respect wherever science is cultivated,—who is here to-night to enjoy the honours due to a long career of persevering devotion to knowledge, and to receive, if he will condescend to do so, from myself, the expression of my own deep personal regret, that increase of years, which to him up to this hour has been but increase of wisdom, should have rendered him, in respect of mere bodily strength, unable to fill on this occasion an office which, in his case, would have received more honour than it could confer. I do regret that any cause should have prevented the present meeting, in his native town, from being associated with the name of Dalton as its president. The council well know my views and wishes in this matter, and that could my services have been available, I would gladly have served as a door-keeper in any house where the father of science in Manchester was enjoying his just pre-eminence."

Dr. Dalton's three last essays, already noticed, appear to have been communicated to the Chemical Section. This was the latest memorable event in his intellectual life. But he continued during the two following years to spend some hours daily in his laboratory; and though he complains, in letters to Sir R. Murchison and other correspondents, that "his memory is gone altogether and his faculties impaired," he yet adds, "I succeed in doing chemical experiments, taking about three or four times the usual time, and I am long in calculating."

It was on the 27th July, 1844, that his malady recurred, and rapidly proved fatal. The following circumstances were communicated to Dr. G. Wilson, at no long interval after his decease.

"On Friday, the 26th of July, he retired to his room about a quarter or twenty minutes after nine o'clock; and going to his desk, on which were usually placed the books in which he recorded his meteorological observations, he entered therein the state of the barometer, thermometer, &c., at nine o'clock; and added in the column for remarks, the words 'little rain,' denoting that but little had fallen during the day. His servant observed that his hand trem-

bled more than he had ever before seen it, and that he could scarcely hold the pen. Indeed, the book exhibits, in its tremulous characters and blotted figures, striking proofs of the rapid decay of the physical powers. But there was the same care and corrective watchfulness as ever, manifested in this his last stroke of the pen; for having written opposite a previous observation, 'little rain this,' he now noticed that the sentence was incomplete, and added the word 'day,' which was the last word that was traced by his tremulous pen. He retired to bed about half past nine, and spent a restless and uneasy night, but seemed, on the whole, in his usual way when his servant left his bed-side at six o'clock next morning.

"About half an hour later, his housekeeper found him in a state of insensibility, and before medical attendance could be procured, though it was immediately sent for, he expired, 'passing away without a struggle or a groan, and imperceptibly, as an infant sinks into sleep.'"*

My highly valued friend, Mr. Joseph A. Ransome, Dalton's medical attendant in later life, has kindly furnished me with the following particulars of his illness and death:—

"On the 18th of April, 1837, I received an urgent summons to Dr. Dalton, before breakfast.

"I found him pale, speechless, and paralyzed on the right side; he appeared to recognize me by attempting to speak, and moving his left hand towards me.

"It appeared that he had risen at his usual time, had dressed without assistance, and had entered in his meteorological journal the temperature, maximum and minimum, of the previous night; his hand had evidently been very unsteady, from the tremulous and indistinct figures. After this I believe that he fell, and was discovered in the state in which I found him on my arrival.

"On the previous evening, he had received a visit from a distinguished chemist, with whom he had a long, animated, and somewhat warm discussion on the subject of chemical

* Dr. Wilson, *British Quarterly Review*, vol. i., p. 192.

notation and symbols; he contending that his own plan of representing the probable position of the atoms of a compound was preferable to the system then and now universally adopted, which merely gives the initials of the elements and the number of their atoms. After the departure of his visitor, he appeared more excited than usual, and unable to let the subject drop and to subside into his usual calm.

"A consultation with the late Dr. Holme and yourself was immediately arranged, at which it was decided that his condition, from great prostration, would not admit of any depletory or reducing measures.

"In a few hours he rallied somewhat, and in a few days recovered, in great measure, the use of the right side, but articulation and the memory of words remained impaired for a much longer time; indeed, I may say they were never completely restored to their former normal condition.

"You may probably remember his impatience at the occasional doses of medicine which were strictly insisted upon by our senior colleague, and the amusing diary with hourly remarks, which were duly kept at his request by his kind and indefatigable friend, the late Mr. Peter Clare, in which were recorded with great minuteness the effects of medicines, his own sensations, &c.

"As nearly as I can recollect, about the end of May I accompanied him in his first drive out, to the Botanical Gardens, which the Council had kindly placed at his disposal as a place where he could take exercise with great privacy during his convalescence.

"During our walk through the conservatories, he conversed freely though somewhat indistinctly with the gardeners, and requested that they would forward to him bottles of air from the greenhouses after prolonged closure.

"In about six weeks after this, he resumed his usual avocations, and excepting for some slight ailment, as catarrh, I discontinued my *professional* attendance.

"In the following year, 1838, on the 15th of February, he had a second slight attack, from which he soon rallied; after which he remained free from any decided attack,

although his strength, particularly his physical, failed so decidedly as to require constant assistance, until May 20, 1844, when another slight fit occurred.

"A few weeks after this, he received a vote of thanks from the Literary and Philosophical Society, for his valuable contributions to its Memoirs, to which he was unable to reply orally, but delivered a written acknowledgment.

"Lastly, on the 27th of July immediately following, I was again hastily summoned, but only to behold the lifeless body of my venerable friend.

"His increasing debility had, of late, rendered incessant attendance necessary, and this duty had been performed in a most exemplary manner by his faithful and attached servant; but on the morning of this last day, after a warning to remain quiet in bed until the return of his attendant, the Doctor had (seemingly in an effort to rise) fallen backwards from the bed, and was found with his head on the floor, quite lifeless.

"On the following day, an examination of the head was made by my friend Mr. Wilson and myself. We found the veins and sinus distended with blood, but there was no evidence of recent rupture of any, even the smallest, vessel. In general, the substance of the brain was firm, but in the anterior portion of the middle lobe on the left side, above the fissure of Silvius, we discovered a sac, firm and rather thick, containing the débris of an old coagulum, and softening of the brain around it. The arteries of the brain were all more or less brittle from earthy deposit, but we did not discover any aneurismal dilatation.

"The weight of the brain and membranes was about 3¼lbs.

"Mr. Bally (formerly assistant to the late Dr. Spurzheim) was present, and took casts of several parts of the brain, and the cavity of the skull. He pointed out a remarkable prominence on the frontal portion of the orbitar plates (which represents the phrenological site of the '*organ of colour*'), and the imperfect or deficient development of the convolution of the anterior lobes, which rested upon

them. Of course, Mr. Bally adopted this as the true explanation of the peculiarity of Dalton's vision; and we as witnesses (not only without faith in 'phrenology,' but even opponents) are bound to record the fact.

"The eyes, in situ, presented no peculiarity; the corneæ had the usual semi-opaque arcus senilis round them, but no discoloration of the humours could be discovered then or subsequently. As their removal and careful examination were necessary to the elucidation of a scientific truth of some importance (with the consent and approbation of one of his executors then present), I removed them for the purpose of making a more careful examination of them at my leisure.

"On examination, I sacrificed one to the determination of the colour of the three humours; the *aqueous* was collected in a watch-glass from a careful puncture of the cornea, and viewed both by reflected and transmitted light, was found to be perfectly pellucid and free from colour. The *vitreous humour and its envelope* (the hyaloid membrane) *were also perfectly colourless.* The *crystalline lens* was slightly amber-coloured, as usual in persons of advanced age. The tunics, retina, choroid and sclerotic, with their subdivisions, presented no peculiarity.

"In the other eye, the posterior part being removed by a vertical section in a plane at right angles with the axis, with as little disturbance as possible of the humours, we were able to see objects as through a lens, and thus objects of different colours, both by transmitted and reflected light, were examined *without any appreciable difference.* I did not omit to place scarlet and green together, as I knew that the Doctor was unable to discover any difference between the colour of the scarlet geranium flower and its leaves; but to my eyes, the contrast of the colours seen through the medium of the greater portion of the humours was as great as ever. Sir David Brewster visited me shortly after this examination, and I endeavoured to keep the humours in a state for his inspection and experiment; but he suggested nothing further, as he agreed with me, that the imperfection of Dalton's

vision arose from some deficient sensorial or perceptive power, rather than from any peculiarity in the eye itself."

There was an universal feeling in the principal inhabitants and municipal authorities of the city of Manchester, to pay honour to the memory of the deceased philosopher, by a signal mark of respect. A public funeral was resolved upon; and with, in my judgment, more questionable propriety, the coffin containing his remains was placed in the Town Hall, in a darkened apartment, hung with black drapery, and illuminated by artificial light. It was visited by upwards of forty thousand spectators. The public funeral took place on Monday, August 12. A procession was formed of nearly a hundred carriages, and many hundred persons on foot; the windows were lined with spectators, as well as the roofs of the houses; nearly all the shops and warehouses in the line of the procession, and many in other parts of the town were closed; four hundred of the police were on duty, each with an emblem of mourning; and the funeral train was about three quarters of a mile in length. Almost every public body in the towns of Manchester and Salford were represented in the procession. He was interred in the Ardwick Cemetery. The grave will be marked by a plain monumental slab of polished granite.

A long and frugal life, and the succession, after his brother's death, to the small family estate, had enabled Dalton to accumulate a moderate fortune. In his will, he had bequeathed the sum of two thousand pounds, "to found, endow, or support a Professorship of Chemistry at Oxford, for the advancement of that science by lectures, in which the Atomic Theory, as propounded by me, together with the subsequent discoveries and elucidations thereof, shall be introduced and explained." But, in the codicil, he revoked this bequest, in order, I am informed by Mr. Neild, to provide more largely for the family of his friend, the Rev. Wm. Johns, who had sustained, in his old age, a heavy pecuniary loss.

Since Dalton's decease, a sum, amounting to about five thousand pounds, has been raised, chiefly in Manchester, for

the object of establishing there some lasting memorial in his honour. It has, I believe, been finally decided to appropriate about one thousand pounds of this subscription to the erection of a bronze statue in the grounds of the Royal Infirmary. Mr. Theed is to be entrusted with the execution of the work, which must necessarily be modelled from Chantrey's marble statue. "It is proposed with the remainder of this fund to establish, in connection with the Owen's College, Manchester, two scholarships in chemistry, to be called 'The Dalton Chemical Scholarships,' each of the annual amount of fifty pounds; two scholarships in mathematics, to be called 'The Dalton Mathematical Scholarships,' each of the annual amount of twenty-five pounds; and an annual prize for students of the Natural History Class of Owen's College, of such amount as the income of the fund may admit of, after providing for the Chemical and Mathematical Scholarships. Each scholarship shall be held for two years, and shall be competed for in alternate years, so that one scholarship may be open to competition every year."*

* Mr. Wm. Neild, the Chairman of the Committee, and one of the most active and zealous promoters of its objects, has since informed me, that the total amount raised is £5,312. The annual surplus remaining after providing for the statue and the scholarships, will be £15, to be applied as a Natural History Prize.

CHAPTER IX.

PERSONAL APPEARANCE—PORTRAITS AND STATUE—MORAL EXCELLENCES—ANECDOTE FROM DR. PLAYFAIR—MISS JOHNS'S NOTICE OF HIS DOMESTIC HABITS AND CHARACTER—EXTRACTS FROM MR. GILES'S SKETCH OF HIS LIFE—NOTICES BY SIR H. DAVY AND DR. DAVY—LETTER FROM MR. RANSOME—GENERAL SURVEY OF HIS MAIN DISCOVERIES AND OF HIS CHARACTERISTIC MENTAL ENDOWMENTS.

Doctor Dalton possessed that healthful masculine organization of mind and of body, which characterizes the natives of our northern counties, and which has, at all times, commanded for them far more than an average proportion of the higher mathematical honours in the University of Cambridge.

He was of middle stature, of vigorous muscular frame, inclined to much bodily exercise, and as his venerable friend, Mr. Jonathan Otley of Keswick, relates, was almost unrivalled as a pedestrian. The strongly defined lineaments of his thoughtful countenance have been fortunately preserved at various periods of his life. A portrait by Allen, in the meeting-room of the Literary and Philosophical Society of Manchester, painted in 1814, represents him in the full vigour of mature manhood. He is more faithfully recalled to the remembrance of his younger friends by the intellectual bust, modelled by Sir Francis Chantrey, in preparation for the statue, and which has been reduced by a mechanical contrivance, and copied in ivory by Mr. Cheverton. In more advanced life, when his features had lost much of their chiselled firmness, and the lower jaw, in place of its former almost severe compression, drooped somewhat, Mr. Phillips made a very successful portrait of him, now in the possession of Mr. Duckworth, of Beechwood. In the form of the forehead, and the upper part of the face, there is the

strongest resemblance to the engraved portrait of Newton; and the members of the British Association, who were present at Cambridge in the year 1833, were impressed with his likeness to Roubiliac's statue of Newton in Trinity College Chapel. Mr. Woolley, who saw a cast of Newton's head placed near Dalton after his decease, has recorded "that the likeness which had been observed during life, was in death most striking."

Doctor Dalton's moral excellences, from his living unmarried and much alone, had a limited field for their manifestation. He enjoyed that equable healthful tone of nerve, of pulse, and of digestion, of which, whether as cause or effect, a serene temper is the usual exponent. He did not possess a lively sensibility; and his outward bearing, even towards his intimate friends, was calm and undemonstrative. But his attachment, when once deliberately bestowed, on the solid ground of esteem for tried worth, or of the common pursuit of the same objects in science, was never weakened or alienated. His friendships were earnest, steadfast, and unalterable; and if need came, were evidenced by acts of thoughtful generosity. One such instance has been communicated to me by a lady, who knew him intimately. "A fire occurred in the works, during my brother's minority. A few days afterwards, Mr. Dalton offered to my mother, all the funds he had saved, if any money was wanted. It was not required; but we thought it an act of very considerate kindness and friendship." His moderate desires, as regards fortune, may be compared with those of the most self-denying of ancient philosophers; and were the more deserving of praise, as he passed the larger portion of his life among a community eagerly engaged in the pursuit of wealth. Many anecdotes have been preserved of the almost ridiculous moderation of his charges for performing chemical analyses. These, which were often merely a few shillings, never, I believe, exceeded a sovereign. He was in the habit of giving his invaluable instruction, in mathematics and chemistry, at the trifling charge of 2*s.* 6*d.* per hour, or, if two or more students attended together, at 1*s.* 6*d.* each for the hour.

In his early days, he had necessarily formed habits of prudent economy, and throughout life his personal wants were few and easily satisfied. I learned from the late Mr. Clare, that when invited to contribute to funds raised by the Society of Friends, or for other general objects, it was his habit to take time for deliberation; but that, if on reflection, he was satisfied with the propriety of the object, he was accustomed to give largely, in proportion to his means. During the latter years of his life, when his resources had become more ample, I have discovered from his papers, that he made a liberal annual allowance to two distant female relatives, for whom he also eventually provided in his will.

An amusing anecdote, as illustrating his simplicity and exactness in money matters, has been communicated to me by my friend Doctor Lyon Playfair. It must, however, be borne in mind, that the incident occurred after his mental faculties had been seriously impaired; indeed, not much more than six months before his death. I have discovered in the letters written by him, at this period, which he had copied, in a tremulous hand, into a rough letter book, painful indications of imbecility and of needless alarm about his pecuniary affairs. It certainly does not consist with my knowledge of his character, which, in his best estate, was always liberal, though frugal.

"At one of the meetings of the Literary and Philosophical Society of Manchester at the end of 1843, or early in 1844, I gave Dalton a small pamphlet, which was a reprint of two lectures, which I had given to the Royal Agricultural Society. A few days after, Dalton sent for Peter Clare, to ask him if he knew what I had given him, as he had mislaid it. Peter Clare assured him he had received nothing from me, so the housekeeper was sent for, and she recollected that he had brought home a book with my name on it, but she was unable to say what it was. Accordingly Peter Clare was sent to my laboratory, as an ambassador, to ascertain what the book was, and *what was its value.*

"As an author, I found some difficulty in estimating the

value of the performance; but was soon relieved by being told, that it was not the scientific, but the *money* value of the present, which Dalton desired to know. I stated that the price, if sold, might have been sixpence or a shilling. In about an hour Peter Clare returned with a message from Dalton, that he had intended to have presented me with all his works, but as their cost was above thirty shillings, he could not think of giving them for a one shilling present. He made, however, the entertaining proposal, that I should gather together all I had ever written, send them to him with a priced list, and then he would make up his mind as to whether he could present me with his works. You may suppose, that I was too much amused with the proposal to hesitate, so I gathered together what I could, including my translation of Liebig's *Agricultural Chemistry*. After this had been done, I still found that the declared value of my present was seventeen shillings less than that offered by Dalton, but nevertheless I boldly sent it. Next day came Peter Clare with all Dalton's Works, every volume having on its blank page in the author's autograph,

John Dalton, D.C.L., F.R.S.,
to
Dr. Lyon Playfair,
Jany. 1844.

"But I was requested forthwith to hand over the 17*s*., which I immediately did, delighted to have the works from the great author, and prizing them the more from the droll characteristic way in which they came into my possession."

No one enjoyed such ample opportunities of becoming, in the habitual intercourse of daily life, intimately acquainted with Dalton's domestic tastes and moral qualities, as Miss Johns, — a highly educated lady, and superior classical scholar,—in whose father's house he resided nearly thirty years. I am greatly indebted to Mr. Woolley, her executor, for permission to publish the subjoined record of her impressions of Dalton's character.

"The doctor's habits of life (writes Miss Johns), were so uniform and unvaried, as to be soon related. On Sundays,

he always dressed himself with the most scrupulous attention to neatness, attended public worship twice, except when indisposed, or on very particular occasions, which, however, the writer does not remember to have occurred a dozen times in all; dined, during *his* life, with his friend Mr. Thomas Hoyle, the printer of Mayfield, and returning home to tea, spent the evening in his philosophical pursuits. His dress was that usually worn by the quakers, avoiding, however, the extreme of formality, and always of the finest texture; hat, gloves, gaiters, and even a handsome cane to correspond. In his general intercourse also, he never adopted their peculiar phraseology. With respect to his religious principles, I should be disposed to think, that he had never made theology, properly so called, a study; he certainly never mentioned having done so, but his reverence for the great Author of all things, was deep and sincere; as also for the scriptures, in which His revealed will is expressed. When the occasion called for it, I have heard him express his sense of the duty and propriety of the religious observance of Sunday, and also his serious disapprobation of its violation. Although frequently solicited, he refused all invitations to dine out on that day, except a very few times at Dr. Henry's, and once or twice elsewhere, when, as he observed to me, he was asked to meet a very distinguished professor, whom he should otherwise have missed the opportunity of seeing. But when the same friend, presuming on his former compliance, again invited him on that day, he received a refusal, which prevented any farther application. His week-days, every day, and all day long, were spent in his laboratory, with the exception of Thursday afternoons, when he accompanied a party of friends, about three miles into the country to bowl, and entered into the amusement with a zest, infinitely amusing to all who were present. He also spent a few minutes, generally between light and dark, at the Portico, in reading the daily papers. He rose about eight in the morning, always lighted his laboratory fire before breakfast, after which meal he finished his toilet and repaired to his laboratory, which he seldom left until dinner. He dined at one, but always came in in much

haste, when dinner was partly over, I suppose, to save time; he ate moderately, and drank only water; he was obliged to eat slowly, on account of the conformation of his throat, which was very narrow. After dinner, he always spent about a quarter, rarely half-an-hour, in chatting with the different members of the family, or any visitor, or in looking over any chance publication lying on the table. After spending the afternoon in his laboratory, he drank tea at five, rarely coming in until the family had nearly finished. He was very methodical in the quantity he took at meals. After tea, to his laboratory again, where he staid until nine (supper-time), when he regularly shut up for the night, ate a light supper, generally of meat and potatoes, until about his sixtieth year, when he changed this for meal porridge, with milk or treacle, or occasionally a couple of eggs. After supper, we all sat together, and generally had a nice chat, for which the labours of the day had excellently prepared us all, and I will venture to say, that few fire-sides have ever presented a scene of more innocent and pleasant recreation, than did ours during these, the busy years of our life. The doctor took little part in the conversation, though he shewed, that he listened, by frequently smiling, and now and then uttering some dry, laconic witticism, in reference to what was passing. He and my father smoked their pipes unremittingly. Not unfrequently, we were joined by two or three political friends, who talked over the news of the time, &c. The doctor enjoyed their society, but took little part in the conversation, in politics none whatever, nor for years had we any idea what his views on that subject were (Conservative). Occasionally, he took the chief part in conversation; but this only, when we were quite alone, or when Mr. Ewart stepped in, as he sometimes did. He and the doctor had a great esteem for each other, which lasted through life. When, however, this gentleman was our visitor, the evening seldom ended without my father and he getting deeply into metaphysics, a favourite study with both. The doctor generally listened intently, but from an occasional ironical smile, I used to suspect that he thought

it mostly, "vain wisdom all and false philosophy." My sister Catharine's wit and animated nonsense, were, I fancy, more to his taste. When we had no company, we always withdrew before eleven, when the doctor pursued his meditations for nearly an hour longer, and then having perambulated the lower part of the house, to see that all the fires were out, he himself went to bed, and by midnight all were at rest.

"I should mention, that when more than usually busy, he could abstract himself completely, and pursued his own reflections without interruption from any noise or talk around him; indeed, on these occasions, we generally left him to himself, after teazing him for a few minutes. Not unfrequently have we been favoured with the company of some of the most distinguished philosophers of Europe, more especially foreigners, and in these cases, he most good-humouredly promoted a general conversation, by saying, 'these ladies will be interested in seeing a Frenchman, an Italian, &c., for they have been reading so and so;' or to my father, 'this gentleman is an eminent classical scholar, has visited Troy, etc.' Then perhaps leaving us, until he had finished some interrupted experiment, or made his arrangements for the evening, we should enjoy a most agreeable hour, enhanced by the reflection, that such a privilege was enjoyed by few. If at leisure, he stayed with us, and listened as usual for some time, to what was passing, then taking advantage of a pause in the conversation, he would ask some scientific question, which of course always led to subjects to him of the deepest interest, but from which we generally withdrew as quite beyond our comprehension. I much wish that I had retained the names of these distinguished persons,—many of them had very barbarous names, not easily understood at the time. Amongst them, I particularly remember M. Biot, as the most interesting person possible, though it must be confessed, that a great deal of the amusement he afforded, was purchased at the expense of truth.* Dr. Hamel, of whom the same might be said,

* Miss Johns probably alludes to some innocent badinage of that distinguished man.

except that he struck us as less distinguished than the former. A Bavarian,* who died in Scotland, poor man! A Pole, from Wilna, who mentioned the distress that the march of Napoleon's army had occasioned, by causing bread to rise to 2*s.* 6*d.* per pound at Wilna. Perhaps I remember most vividly the visit of Dr. Forchhammer, a Dane, a most sensible and agreeable person, whom my father thought the most accomplished person he had ever met with, and whom the doctor described as profoundly versed in chemistry and other branches of natural philosophy. This gentleman was at that time only twenty-five years old. The doctor, after his illness particularly, took great pleasure in recalling the remembrance of the distinguished persons, both at home and abroad, whom he had had the privilege of knowing and conversing with. One evening, he mentioned the names of several of his own countrymen; Sir Humphry Davy, Dr. Wollaston, whom he particularly admired and esteemed, Dr. Roget, and others, whose names I cannot now recall. He mentioned his regret at not having been introduced to Cavendish, who was of a particularly shy and reserved nature. But his visit to Paris, in 1822, perhaps afforded him as much, if not more pleasure, than any circumstance in his life.

"In that city he was received with a distinction, and met with those attentions which at that time had not been accorded to him in his own country, and he ever retained a most grateful and affectionate remembrance of those who had shown a sense of his merit.

"From those eminent philosophers, Cuvier and La Place, he met with the utmost kindness and hospitality; besides these, I have heard him mention with much regard Berthollet, Thénard, and a Dr. Edwards. At the houses of Cuvier and and La Place he met with company exactly to his taste; but perhaps more than these, did he value the acquaintance and cherish the memory, of the interesting and accomplished Clémentine Cuvier, daughter of the great naturalist, whom he ever after mentioned as the most attractive and amiable

* M. Yelin.

young creature that he had ever seen, and whose early death he sensibly lamented. For some time after his return from Paris, he exhibited an unwonted degree of vivacity and communicativeness, and we frequently bantered him with having become half a Frenchman. Generally speaking, however, the manners and habits of the Parisians were little to his taste, and they must have gazed with some surprise on a great man of an appearance so simple and unpretending, and who had more than the usual portion of English quiet and reserve in his manner. The mention of Clémentine Cuvier leads me to the subject of his sentiments towards the female sex, which were common to him with *almost* all great and good men. He preferred their society—for their understandings he ever professed the highest respect—his warmest friendships were, to the end of his life, with individuals of that sex, and on all occasions he shewed them those little delicate attentions which are particularly acceptable to them.

"As one instance of this kind, I may mention, that my sister and myself never left home, be the hour ever so untimely, that he did not see us into the coach and safe off. But not only to us, to the servants also did he pay the same kind attentions on their annual visits to see their friends. These servants had lived for many years in the family, of which they were considered and treated as a part. His conduct to them was uniformly kind, considerate, and liberal. When we left Manchester, before taking leave, he called these two women, and highly praising their fidelity and constancy in remaining for so many years in the same place he gave to the elder one two, and to the younger one, one sovereign.

"He used to mention with the warmest interest, and with deep sensibility, a most amiable and accomplished quaker friend, who died young, but whose memory he ever cherished with the fondest regret. There was nothing but friendship in this, as she was already engaged when he became acquainted with her. He had a letter and some verses of this lady's, with which we could by no means prevail upon him to part, or even to let us look at, though he read them to us with a faltering voice, and what was very rare indeed with him,

with eyes suffused with tears, repeating as he ended, 'Poor Nancy! Poor Nancy!'"

I have been allowed to introduce the following extracts from a sketch of the life, character, and discoveries of the late John Dalton, by Mr. Samuel Giles, member of the Literary and Philosophical Society of Manchester; and formerly one of his pupils.

"His voice was deep and gruff, and his articulation thick, indistinct, and mumbling; so much so as to give a stranger almost the idea of uncouthness or moroseness. This may have arisen in part from his having spent his boyhood in a country village amongst the agricultural population, and also from his elocution having been neglected in his early education.

"Dr. Dalton's manners were extremely simple, and as might be expected from such a mind as his, were free from all affectation, pedantry, and ostentation . . . . . In common intercourse he adopted the phraseology of the Society of Friends, and to strangers his manners would often appear repulsive from his entire disregard of the modes of address and expression which the conventional laws of courtesy enjoin, and also from the peculiarity of his voice, and the defect in his elocution. Amongst his intimate friends in the social circle Dr. Dalton was exceedingly cheerful and facetious; he had a keen relish for the ludicrous, and thoroughly enjoyed a witty or amusing anecdote. He was fond of society, and loved sometimes to unbend in the afternoon or evening, for an hour or two with intimate friends, from the severe study to which he addicted himself.

"For several years of his life he was in the habit every Thursday afternoon, when the weather permitted, of taking exercise in the open air, and of spending a few hours in company with a few intimate friends, in the enjoyment of his favourite diversion of bowling . . . . . On these occasions his spirits were buoyant and cheerful, and he entered into the sport with all the keen relish of boyhood. Sometimes, when a fall of snow had taken place, he has been known to request that the snow might be swept from the

bowling green that he might not be disappointed of his game. When it came to his turn to bowl, he threw his whole soul into his game, and after he had delivered the bowl from his hand, it was not a little amusing to spectators to see him running after it across the green, stooping down as if talking to the ball, and waving his hands from one side to the other exactly as he wished the bias of the ball to be, and manifesting the most intense interest in its coming near to the point at which he aimed. A small sum, a few pence, was played for each game, in order to pay for the use of the green, and Dalton set down in his pocket-book with the minutest accuracy, the amount of his losses or gains.

"In his habits and manner of life, Dr. Dalton was extremely simple, regular, methodical, orderly, and temperate. He rose regularly at seven o'clock, and a few minutes after eight the writer has often seen him going across from his lodgings in George-street, or his house in Faulkner-street, which were close by (like Diogenes with lighted lantern in hand) to the rooms of the Literary and Philosophical Society. After having lighted his own fire, and dusted down the desk for his pupils, he went back to his breakfast, and was ready for his pupils at nine o'clock. From nine till twelve, or nearly one o'clock, he devoted to teaching, or in pursuing his own investigations; and frequently after giving his pupils a mathematical or chemical problem to solve, he would go on with his own pursuits until the pupils required explanation, or had finished the problem they were working out. Between twelve and one o'clock he almost daily visited the Portico newsroom, and library, in order to watch the progress of political events, or to read any notices on scientific subjects.

"At one o'clock he went to his dinner; and after a temperate meal, at which he seldom drank anything but water, he returned to his laboratory at two o'clock, and pursued his private teaching or his own experiments till five o'clock, and generally visited the Portico a second time in the day, before or after an early tea. At six o'clock he returned to his laboratory, until about half past eight or nine o'clock, unless he was spending the evening with a friend; and then

after a light supper, consisting generally the last few years of his life, of sago or oatmeal porridge, and after smoking a pipe, retired to rest at ten, or at latest eleven o'clock.

"On one occasion the celebrated Dr. Chalmers visited Manchester, in order to preach for some public object, and requested the gentleman at whose house he was stopping to give him an introduction to the eminent chemist. . . . . At their interview the subject of conversation was the Mosaic account of the creation, and Mr. Walter Crum informed the writer that Dalton expressed himself to be much gratified to find that Dr. Chalmers's views were enlarged and scientific, and not those which many divines held, viz., that the matter of which the world is made, as well as the form it now assumes, was created only 4,000 years before the Christian era.

"Dr. Dalton was always very reserved upon religious topics, and was scarcely ever heard to allude to religion. He was a member of the Society of Friends, and regularly attended their meetings twice every Sunday. There is little doubt that he was a firm believer in the truths of divine revelation as contained in the Holy Scriptures, but it was difficult, if not impossible, to ascertain what were his doctrinal views. He was scarcely ever seen to be engaged externally in any devotional exercise, social or private; when, however, he was prevented by illness from attending the Friends' meetings on Sunday, and his housekeeper, Miss Wood, occasionally proposed to read the Bible to him, he assented, and on her reading it, seemed much gratified, and always listened with the greatest reverence and attention."

For the following notices of Dalton, hitherto unpublished, from the pen of Sir H. Davy and of Dr. Davy I am indebted to the friendly courtesy of the latter. "The first bears date, Rome, February 1829, shortly before Sir H. Davy's last illness. He then amused himself in sketching the characters of some of his distinguished contemporaries."

"John Dalton was a very singular man. A Quaker by profession and practice; he had none of the manners or ways of the world. A tolerable mathematician, he gained

his livelihood I believe by teaching mathematics to young people. He pursued science always with mathematical views. He seemed little attentive to the labours of others, except when they countenanced or confirmed his own ideas. It is difficult to say how he gained his first notions of atoms, but I strongly suspect that *Researches Chemical and Philosophical,* published in 1801, in which it is stated that nitrate of ammonia becomes water and nitrous oxide, and perhaps Cruickshank's discovery of gaseous oxide of carbon gave him his first ideas. He always referred to those labours in his early papers, but afterwards seemed to have forgotten them. He probably had seen the works of the two Higginses, but I do not think he was acquainted with the views of Richter. In his lectures he used to quote the *Optics* of Newton, saying that Newton had expressed his views almost as well as he could express them himself. Whatever came into his mind, from any source, he seemed always to consider his own property. He was a very disinterested man, and had no ambition beyond that of being thought a great philosopher. He was a very coarse experimenter, and almost always found the results he required, trusting to his head rather than to his hands. Memory and observation were subordinate qualities in his mind; he followed with ardour analogies and inductions, and however his claims to originality may admit of question, I have no doubt that he was one of the most original philosophers of his time, and one of the most ingenious."

The next extract was written by Dr. Davy, at Malta, about 1830—31, and conveys his impressions of Dalton, as he knew him in London in 1809—10.

"Mr. Dalton's aspect and manner were repulsive. There was no gracefulness belonging to him. His voice was harsh and brawling; his gait stiff and awkward; his style of writing and conversation dry and almost crabbed. In person he was tall, bony, and slender. He never could learn to swim: on investigating this circumstance he found that his spec. grav. as a mass was greater than that of water; and he mentioned this in his lectures on natural philosophy in

illustration of the capability of different persons for attaining the art of swimming. Independence and simplicity of manner and originality were his best qualities. Though in comparatively humble circumstances he maintained the dignity of the philosophical character. As the first distinct promulgator of the doctrine that the elements of bodies unite in definite proportions to form chemical compounds, he has acquired an undying fame. This doctrine has happily taken the place of that of Berthollet, of an opposite kind, that the proportions in which the elements unite are not definite, and that chemical combination is a complex problem, the result of the operation of many contending powers."

My friend Mr. Ransome has, at my request, recorded, in the subjoined valuable letter, his reminiscences of Dalton, with whom he and his father stood in most intimate relations.

"My dear Henry,—In compliance with your request, I send a few particulars connected with the health of our late venerable friend, Dr. Dalton.

"During the early period of my acquaintance with him (which commenced in 1819), he appeared to enjoy robust health, with the occasional exception of catarrh. He was however very susceptible of the poison of lead, which affected him more directly than most others. During his visits to the metropolis, he discovered very early the effect of the water kept in leaden cisterns, which produced numbness and discomfort in his extremities. The presence of lead in his beverage he at once discovered by chemical tests, and he was almost nervous when water was offered to him until he could ascertain its purity (unless he was satisfied that it had not been contaminated by contact with lead).

"On one occasion when he was suffering from catarrh, this dread (of the effect of metals upon his system) was shown in a somewhat ludicrous manner. My father had been summoned to attend him, and prescribed a small dose of James's powder to be taken at bed-time. On the following day my father called upon him again, and finding him very much better attributed the improvement to the effect of the

medicine, upon which Dalton remarked, 'I do not well see how that can be, as I kept the powder until I could have an opportunity of analysing it.'

"For slighter attacks of cold he was in the habit of mixing a compound of extract of liquorice, treacle, and vinegar, with or without paregoric, in a pan over his laboratory fire, whilst engaged in teaching his pupils; and would descant earnestly upon the virtues of this panacea. Such however were the regularity and temperance of his habits that he enjoyed robust health; and although his occupation was sedentary or, more strictly, confining, he availed himself of occasional holidays. Every week he enjoyed his half-holiday with the Bowling Club, and on Sundays most regularly walked out to his old friend's, the late Mr. Hoyle's house at Mayfield (at that time a pleasant country residence).

"He also made longer excursions during vacations, generally in the direction of the English lake district and sometimes into Derbyshire. In one of the latter it was my privilege to accompany him; although about thirty years have elapsed since it took place, the incidents of the day made so deep an impression upon my mind that I think I can give you even now a tolerably correct narrative of them.

"During Whitweek, which is kept as a general holiday in this district, he decided to make a pedestrian excursion in the north of Derbyshire, to ascend Kinder Scout, the highest hill in the county, and to ascertain the height of it by the barometer.

"On a fine spring morning, Mr. Dalton, provided with a thermometer and a home-made barometer (an old-fashioned oval bulb tube let into a groove in a piece of soft deal, *graduated with ink upon the* WOOD), with myself as his companion, started at an early hour by coach to Chapel-en-le-Frith, a town a few miles distant from the hill.

"We had here the choice of a tolerably direct but cross-country walk or of following the high-road to Hayfield, a village near the base of Kinder Scout; and of these he decided upon the latter, as he did not repose much confidence

in me as a guide along the first route, and had a wholesome fear of travelling over hills without a competent guide. During our walk he was very cheerful, resisting any attempt on my part at conversation on science, but like a schoolboy enjoying a holiday, mocking the cuckoos, putting up and chasing the hares, stopping from time to time to point out some beautiful view, or loitering to chat with passing pedestrians. Having engaged a guide at Hayfield, he then entered into conversation with him on the subject of trout-fishing, the best streams, the quality and size of the fish at different heights, the best baits, &c., which induced me to regard him as a disciple of Isaac Walton.

"At different times during our walk he had made occasional observations with his barometer, and taken the temperature of several springs, explaining to me that the main temperature of a district can be obtained by that of springs.

"Having reached the summit, the barometer was suspended and the depression of the mercury was taken as accurately as its homely construction would permit, and after a few minutes abstraction and calculations on the back of a letter, he announced that probably the height of the hill might be *about* 700 yards, which I believe has been confirmed by the trigonometrical estimate of the Ordnance surveyors. After a hearty dinner at the inn at Hayfield, which he pronounced to be the best and cheapest he had ever made, we set out in the direction of Disley, where we should fall into the track of the Buxton coaches.

"During this walk he gave me much good and paternal advice, advancing the opinion, which he held in common with Sir I. Newton, that there was no such thing as genius, and that if he had accomplished anything which the world considered valuable, it had been done by persevering industry directed to a single practicable object. This led him on to speak of the atomic theory; he said that the germ of this presented itself to him, when still young, before he had undertaken the special study of chemistry. It had occurred to him that if the ultimate particles of matter were hard and

indestructible, as there appeared to be many possessing different qualities, recognised as simple elements by chemists, so might they vary in size or weight or in both. At this stage of our conversation, he illustrated the subject by picking up from the road a piece of limestone, of about a pound weight, asking me, 'Of what is this composed?' I answered, 'Of lime and carbonic acid.' 'In what proportions?' (At that time I gave his own equivalent numbers, but now I prefer adopting the more correct numbers as pretty generally recognised.) 'In 100 parts, 56 are lime and 44 carbonic acid;' or if the weight of the stone is 100 pounds, 56 pounds of the former with 44 pounds of the latter. He then broke the stone, repeating his questions, my reply was that I could not now give the absolute quantities of its constituents, but that relatively they remained the same. 'Exactly so,' he exclaimed, 'and so they would if we could reduce them to the finest dust, of which each particle could preserve its characteristic qualities of a compound, that particle would still consist of these (lime and carbonic acid) in the same ratio.' He then stated that limestone was a compound of at least three elements, and only sufficed as an illustration of the existence of a definite ratio in the proportion of acid and base; and that it would be necessary to discover some simpler compounds in which only two elements existed, and to assume that some of these were binary compounds, or in other words required one atom of each simple element to make up the compound atom; and further to adopt one simple element as the standard of unity, to which others might be referred, and that this latter should be extensively capable of combination in binary, ternary, quaternary, &c. forms. Hydrogen he had selected as his unit, because it existed extensively combined in nature, was capable of numerous combinations, and was the lightest known body, and in the extensively diffused element of water hydrogen was an essential constituent. He next assumed that water was a *binary* compound, or was composed of one atom of hydrogen with one atom of oxygen. Now, by analysis, water contains per cent. 88,88 of oxygen and

11,11 of hydrogen, or in the ratio of 8 to 1, which gives the relative weights of the atoms—oxygen 8 to hydrogen 1.

"He next spoke of the carburetted hydrogens, and stated that in the olefiant, he found the proportion of hydrogen to carbon as 1 to 6, and in the marsh gas as 1 to 3. This was a difficulty as to which was the binary or the ternary compound; but upon going back to the compound of carbon with oxygen, he then found that in carbonic oxide, the proportion of the carbon to the oxygen was as 6 to 8, and in carbonic acid as 6 to 16. He then stated, that from these results, he considered water, olefiant and carbonic oxide as binary compounds, and marsh gas and carbonic acid as ternary, and that the ratio of the weights was hydrogen 1, carbon 6, oxygen 8.

"He concluded with a few remarks on the ternary compounds, and alluded to the peroxide of hydrogen as one; pointing out *that in these the principle of* MULTIPLE PROPORTION *existed,* for as he said, with great naïveté, 'thou knows IT MUST BE SO, for no man can *split an atom.*'* I ventured, however, to allude to the sesquioxides; but was at once silenced by his answer, 'Yes; but THEY are 3 atoms to 2.'

"I dare not assert, after an interval of so many years, that the foregoing conversation represented the process by which he followed up his original conception of the atomic theory, but such was, and still is my opinion. Possibly he might select such illustrations, derived from subsequent experience and reasoning, as might present the subject in a strong and simple manner, as best calculated to make a lasting impression on the mind of his young companion; if so, he certainly succeeded, for, so long as my faculties are spared, I shall never forget this (to me) memorable day.

"You may recollect the opinion that prevailed among his friends, that his face was very similar to that of Sir Isaac Newton: and as a proof that this was well-founded, I narrate the following anecdote. I should premise, that Dalton resisted every entreaty to submit to have a mould

* This is a striking illustration of the *à priori* tendencies of Dalton's mind.

taken from his face, which was urged, but in vain, by Chantrey, when engaged upon his statue.

"One evening I called upon our venerable friend, whom I found seated as usual, with his cat upon his knee, and his newspaper in hand. After the usual salutation, I espied upon a table close to my chair, a plaster mask, which I took up, and forthwith rallied him upon overcoming his prejudices, and submission to the process, adding, that I was rejoiced to find that he would leave us an accurate copy of his features. 'No,' he said, 'it is not intended for me.' I replied, 'that if not a cast, it must be an admirable model, intended for him.' Again, 'No!' Then calling to mind the supposed similarity, I remarked 'that it must be either Newton or himself.' During the period of my uncertainty he was chuckling and much amused, and then replied, 'Yes; it is a cast of Newton's face, which some friend has kindly sent me as a present.' I can only assure one who knows me so well as yourself, that this remark of mine was honest and *bonâ fide*, the impression of the moment, and unconnected with any attempt to flatter him.

"I must conclude this portion of my communication, by replying to your request that I would give you my opinion as to his position in the scientific world as a chemical philosopher, and as a practical chemist. In the former capacity he stands without a rival, for his endeavour was always to connect chemical with physical science. To him we undoubtedly owe a debt of gratitude for the exposition of a simple law which connects with, and in fact introduces chemistry into, the category of the exact sciences. It is true that his determination of the ratio of atomic weights is not, and never was received as true; yet even with errors of experiment, his active cotemporaries discovered a truth which their better training and skill as practical chemists speedily developed. In the art of chemistry, Dalton was deficient; his experiments were not made with accuracy; his very apparatus was too simple, even clumsy, and the results inaccurate: in fact, I believe that he was self-taught, and was deficient in the ART *of chemistry* in consequence. He was

not a great reader; indeed, I have heard him declare on a public occasion, that he could carry his library on his back, and yet had not read half of the books which constituted it. In the preface to the second part of his *New System of Chemical Philosophy*, he declares that his reason for undertaking original experiments is, that 'having been in my progress so often misled, by taking for granted the results of others, I have determined to write as little as possible but what I can attest by my own experience.' And in the next sentence he, with honest pride, expresses an opinion, with which I entirely concur, that it 'will be found to contain more original facts and experiments, than any other of its size, on the elementary principles of chemistry.' To this, in all humility, allow me to add, that though to modern notions very inaccurate, his volumes are full of suggestions, both philosophical and practical. With all their faults, they are a most wonderful example of patient perseverance to provide facts for, and support to, his grand discovery. The readiness with which his early doctrines were received by Thomson, Wollaston, Berzelius, some of the French chemists, and last, not least, your own father, who repeated the experiments with greater accuracy, and adopted more correct results, was, after the first enunciation of the new law, the main cause of its universal adoption."

It seems expedient to attempt, in conclusion, a brief survey of Dalton's main discoveries in science; and a general portraiture, however imperfect, of those original gifts and acquired habits of mind, by which they were achieved.

His earliest tastes and training had taken the direction of the pure and mixed mathematics; and the meritorious researches of Mr. Wilkinson have shown, that between the years 1783 and 1795, from his seventeenth to his twenty-ninth year, he continued to cultivate those sciences with great ardour and success. His numerous contributions to the two Diaries, during that period, furnish sufficient evidence of the soundness and extent of his mathematical

attainments, of the native vigour of his mind, and of his steady perseverance in confronting difficulties. For his earlier solutions were not deemed worthy of insertion, either as Mr. Wilkinson conjectures, from his "inexperience in the art of getting them up," or from his having adopted the algebraical, in place of the geometrical method, which was in especial favour with the editors of those mathematical periodicals. He had made himself master of Fluxions, and was well read in the writings of the English mathematicians. He ceased to contribute to the *Diaries* in the year 1795; and, after that period, his philosophical and chemical investigations withdrew him from purely mathematical studies. He was not, at any time, I apprehend, entitled to rank among profound mathematicians. I have reason to know, that he never studied the differential and integral calculus; and that the writings of the modern French mathematicians were therefore sealed to him. His mathematical knowledge was, however, sufficiently comprehensive for all the practical applications, in general physics, to which he directed it; and its salutary and suggestive influence may be traced throughout all his original efforts in meteorology, and even in chemistry.

His first appearance, among practical workers in science, was 1787, as a meteorologist. In this capacity, he is to be praised, rather for the patient continuity of his observations, during a period of not less than fifty-seven years, than for their extreme precision. His barometers, made, we have seen, by his own hands, were not filled with the precautions absolutely necessary to exclude air and moisture; nor was the mechanical adjustment of scale and basin, such as to ensure accuracy.* His thermometers, too, were at this

* In his latest *Meteorological Paper*, 1840, he states, "The same barometer has been used during the last twenty-two years, as was in use the fifteen preceding years. It stands nearly 1-10th of an inch higher than other good barometers do in the same situation, probably from some difference in the mercury. I find the allowance I have made in the scale for the rise and fall of the mercury in the reservoir is rather too small; consequently, the extreme elevations and depressions are not quite so great as they ought to be; but the influence of this circumstance upon the averages is scarcely worth notice."—*Manchester Memoirs*, vol. vi. p. 565

period, very imperfect instruments; and his first hygroscopes were singularly rude. Afterwards, it is true, he arrived at an exact mode of ascertaining the amount of aqueous vapour. His early and life-long observations on the variations, in weight, temperature, and moisture of the atmosphere, derive therefore their main value from the trains of original thought and the tracks of experimental research of which they were suggestive. These inquiries revealed to him the cardinal truths and fundamental principles of meteorological science, "a branch of natural history, where," Playfair affirms, "it is more easy to accumulate facts, and more difficult to ascertain principles than in any other." Thus his actual measurement of the force of aqueous vapour, at temperatures between 32° and 212° Fahrenheit; and his discovery, that the amount of evaporation from water, at any temperature, the state of rest or motion of the surrounding air being constant, is proportional to the force of its vapour, at that temperature, minus the force of vapour present in the atmosphere, and ascertainable by taking the dew-point, first raised hygrometry to the rank of an exact science, and enabled Biot to inscribe its laws in the language of rigid analytical formulæ. For, from these data, it first became practicable to calculate the weight of aqueous vapour, present in a given volume of air; and the mere relative indications of hygroscopes gave place to absolute determinations of ponderable quantities. It was Dalton, too, who by proving that the weight of steam, in contact with water, present in a given volume of any gas or of a vacuum, at the same temperature, is a constant quantity, finally and for ever demonstrated the unsoundness of the doctrine, that water is dissolved in air by chemical affinity. His original speculations, on the constitution of the atmosphere and of mixed gases, have not been borne out by the latest eudiometrical analyses of air, from different elevations, and have been rejected, on theoretical grounds, by Laplace. But, whatever may be the result of multiplied experiments on air, from the loftiest heights attainable by man; whether, with certain concessions made by Dalton himself, as that the gases are

not absolutely vacua to each other, but offer some mechanical obstacles to inter-diffusion,—his modified doctrine may be still maintained, or must merge in some more comprehensive Law of Adhesion; it undoubtedly called forth, at the time of its announcement, many of the most vigorous thinkers in Europe, became the subject of eager controversy, and thus materially advanced the progress of science. His careful and almost life-long observations on the Aurora Borealis and its relations to magnetism; and his several attempts to measure its altitude, above the earth's surface, must not be omitted from an enumeration of his services to meteorology.

Dalton had early sounded the depths of several of the most profound questions in a nearly allied branch of science —the philosophy of heat. He had especially meditated long and intensely on the theory of specific heat, and on the complex and still unresolved problem of the measure of temperature. As respects the first, he had arrived, with remarkable sagacity, soon after the commencement of this century, at the inferences; 1stly, from general reasoning and a few facts, that the capacities of bodies for heat are not constant, but increase with their temperature; and 2ndly, from hypothetical relations of the molecules of elastic fluids to heat, that the quantity of heat belonging to the ultimate particles of all elastic fluids must be the same under the same pressure and temperature; both which inferences have been since signally confirmed, and as respects the second, extended to the atoms of matter in the solid and liquid states. He measured, simultaneously with Gay Lussac, and with almost equal exactitude, the expansion of the gases by heat; and showed that it is identical in amount for all the permanent gases. He thus supplied an argument in favour of the *hypothesis*, afterwards proposed by Petit and Dulong, that the gases expand equally for equal increments of temperature, and that their expansions constitute an absolutely exact measure of temperature,—though he himself first conjectured that air expands as the cube of the temperature from absolute privation; and afterwards taught, that the expansion of permanent elastic fluids is in *geometrical* pro-

gression to equal increments of temperature. This, and three other supposed laws of temperature, announced by Dalton, were, it is true, proved by the French philosophers to be hasty generalizations from too limited a range of experiment; but they were of value as points of departure for their own more refined and exact researches. Dalton's thoughtful and suggestive chapters on heat contain, indeed, a mine of intellectual wealth, which, it is manifest, had been thoroughly explored by the distinguished men who followed and passed beyond him in this path of physical science. Even now, I conceive, that an inquirer could not better gird himself for active labour, in this difficult branch of physics, than by the careful study of the early chapters of the first part of the *New System of Chemical Philosophy*. These discoveries, all out of the pale of chemistry, would, of themselves, suffice for the lasting glory of one man.

Dalton had been engaged upwards of sixteen years in contending with these difficult problems in the higher philosophy of heat and of meteorology,* before he entered the field in which he was destined to reap his most abundant harvest of fame. He first appeared among chemists as the author of the atomic theory: the earliest indication to the world that he had devoted any attention to chemistry being the table of atomic weights, explained in a few sentences, subjoined to his memoir, on the absorption of gases by water: perhaps the most significant announcement, in the briefest words, to be found in the history of physical science. We have here a most signal instance of the inborn tendency of Dalton's mind, an instinct strengthened and sharpened by

* Professor James Forbes has erred in associating Dalton with "philosophers who, in pursuit of other objects, have stepped aside from their systematic studies and bestowed upon the science of meteorology some permanent mark of their casual notice of a subject they never intended to prosecute, and which they soon deserted for other and more favoured paths of inquiry. Mr. Dalton descends for a moment from his chemistry in the abstract to illustrate the constitution of the atmosphere and the theory of vapour."—*Report on Meteorology*, p. 197. Dalton's course, in point of fact, was exactly the reverse. He was primarily and permanently a meteorologist; and I have endeavoured to demonstrate, that he became a great chemist in *consequence* of being a meteorologist."

early mathematical training, to soar at once to lofty and abstract generalization. It is a conviction, forcibly impressed on me by the study of all his great monographs, and confirmed by occasional utterances from his own lips, that the order of mental processes, in his course of discovery, was, first, patient protracted meditation, suggesting the probable existence of a general relation or law; and secondly, experiments, more or less accurate and full, devised to test the soundness of this imagined relation. Dalton probably never instituted a single experiment without a clearly preconceived object; certainly never, like Priestley,* made random assemblages of chemical agents, in the hope that some new combination might be fortuitously produced. It may be urged, that this is nothing more than the recognized and legitimate province of hypothesis, and that the same parentage of experimental researches may be traced in the working of all great discoverers. In the words of Dugald Stewart,† "It has probably been in this way that most discoveries have been made; for although a knowledge of facts must be prior to the formation of a legitimate theory, yet a hypothetical theory is generally the best guide to the knowledge of connected and of useful facts." All that is here maintained is, that, in the achievement of Dalton's discoveries, the antecedent state of persistent meditation, and reasonings based on abstract mathematical relations, bore emphatically the largest share, and that his subsequent experimental labours were comparatively secondary and subordinate.

It is certain, at least, that, in the didactic exposition of his discoveries, he has almost uniformly preferred the synthetic to the analytic method. Thus, in most of his memoirs,

* "Finding by experience how much chance had to do with the success of his investigations, he resolved to multiply experiments, with the view of increasing the numerical probabilities of discovery. We find him confessing on one occasion, that he was led on by a random expectation of some change or other taking place."—Dr. Henry's *Estimate of the Philosophical Character of Dr. Priestley. Br. Assoc. Reports*, vol. i., p. 64.

† *Use and Abuse of Hypotheses. Phil. of the Human Mind*, vol. ii. p. 429.

as in the four great essays on mixed gases, evaporation, &c., he commences with the statement of general laws, and the experiments, of which those laws are the expression, are described afterwards, and not then even with the minute detail now deemed indispensable. Again, in the short but most important chapter on chemical synthesis, in which he first gave to the world his own authorized version of the atomic doctrine, he does not broadly and distinctly derive that theory by inferential induction from the ascertained facts of definite and multiple proportion; but rather propounds it, on its own merits, almost as an incontrovertible law of nature,* and lays down seven general *à priori* rules "as guides in all our investigations respecting chemical synthesis."

This prominent tendency of Dalton's mind to constrain complex empirical phenomena, within the rigorous limits of mathematical expression, is manifested no less signally in his failures than in his brilliant and lasting triumphs.† It is remarkably exemplified in his unsuccessful attempt to comprehend, within simple numerical relations, phenomena so eminently complex as the expansions of homogeneous liquids and the force of steam from pure liquids; and, in the case of gases absorbed by water, to establish the existence of a simple ratio between the distances of the particles within and without the liquid.

It would be unadvised to claim for Dalton a high station in experimental chemistry. He possessed neither the mental habits and tastes for extreme exactitude, nor the unrivalled manual expertness which characterized Davy, Wollaston, and Prout. He was never solicitous to ensure the absolute

* I am even inclined to suspect, that the framing of the atomic hypothesis may have been the antecedent, and the discovery of multiple proportions the consequent, rather than the converse.—See his words reported by Mr. Ransome. I have no hesitation in affirming, that this was the order of sequence, as regards the law of compound proportions,—obviously of atomic parentage.

† "Nor is the utility of hypothetical theories confined to those cases in which they have been confirmed by subsequent researches; it may be equally great where they have completely disappointed the expectations of their authors."—Dugald Stewart, vol. ii. p. 429.

chemical purity of the substances which he analyzed, nor did he ever possess a balance of superior delicacy. Most of his chemical researches were investigations into combining proportions, in order to obtain the atomic weights of bodies. The numbers he obtained differ largely from those ascertained by Berzelius and other exact experimenters, and were never accepted by chemists. Yet his own confidence in them long continued unshaken. Relying upon his very imperfect experiments on gaseous combination, he never assented to Gay Lussac's beautiful Law of Volumes; nor in later life did he ever recognize the truthfulness of Liebig's refined process of organic analysis. Thus his habitual and exclusive dependence upon his own robust mind, and his practice of interrogating nature rather than books, and a certain unbendingness of will, which in a less highly gifted nature might have verged upon obstinacy,—qualities of temper, however, which in him did not exclude entire modesty in bearing and in self-appreciation,—while they were the mainsprings of his original discoveries, certainly limited the range of his sympathies, and rendered his judgments one-sided, and occasionally even unsound.

As an experimenter, Dalton must then be placed far below his great contemporaries, Wollaston, Prout, Davy, Gay Lussac, and Berzelius. Yet to a just appreciation of his merits even in experimental chemistry, it is essential to have regard to the objects he had in view. Profound meditation on the constitution of elastic fluids had suggested to him the conception that they consist of solid molecules or atoms, rendered self-repulsive by surrounding atmospheres of heat. When, in the subsequent course of his studies, his attention was directed to chemistry, an intuition of genius called up before him his previous physical conception of atoms, as furnishing an interpretation of the phenomena of definite proportions. Dalton, therefore, entered the field of chemical analysis, in quest of evidence to establish this grand idea. His aim was to assemble rapidly a multitude of examples, especially of the significant class of multiple proportions. Precise numerical exactitude, however abstractedly desirable,

was not an essential requirement. The broad principle was equally brought out by his loose experiments, that determined the atomic weights of oxygen and azote to be respectively seven and five times that of hydrogen, as by the rigorous determinations of modern experience. It cannot be too emphatically stated, that in chemistry, and in every allied branch of science which he cultivated, Dalton valued detailed facts mainly, if not solely, as the stepping-stones to comprehensive generalizations.

The greatest of these, his Atomic Theory of Chemical Combination, is still, and—as being from its essential nature unsusceptible of peremptory experimental proof—must ever remain, an hypothesis. But it is an hypothesis which has already outlived half of a century, eminently fertile in great chemical discoveries. These, indeed, so far from disturbing, have hitherto served only to enlarge and strengthen its foundations. Thus, in general physics, Dulong has shown, that equal specific heats are attached to precisely those relative weights of various elementary bodies, which on purely chemical grounds had been inferred to represent their atomic weights; and Mr. Faraday has disclosed the existence of a parallel identity of quantity in the *electricity* associated with the chemical atoms. Mitscherlich's law of Isomorphism demonstrates the identity of crystalline form of compounds analogous in *atomic constitution,* and has itself recently received fresh significance from the striking observations of Kopp and Schröder, which show identity of *atomic volume* in elements, that give birth to isomorphous compounds. In pure chemistry; isomerism with its sub-genera, polymerism and metamerism—or the co-existence, in numerous series of bodies, of dissimilar physical and chemical properties, with identity of chemical composition (as estimated in the percentage proportions of their constituents), admits of explanation, in the present state of our knowledge, only on the supposition of differences in grouping of elementary or compound proximate *atoms.* Finally, in the vast domain of organic chemistry, where the atomic theory is brought in presence of a new and boundless series of complex compounds

wholly unknown to its author, we have seen that the universal prevalence of rational atomic formulæ, the remarkable laws of substitution and of homologues, and the doctrine of chemical types, all necessarily imply the admission of the atomic hypothesis. These most impressive analogies, with the great empirical laws of definite and multiple proportions, —in themselves ultimate and inexplicable facts,—but clear and significant "*φωνᾶντα συνετοῖσιν*," in the light of the atomic philosophy, *almost* embolden me to claim for it the rank of a "legitimate theory," and for its author a place among the "Repertores Doctrinarum," not far below that of the greatest ancient atomic teacher, Epicurus,—

> Qui genus humanum ingenio superavit, et omneis
> Restinxit, stellas exortus uti aërius sol.

Even as a convenient and provisional hypothesis, the chemist will hardly be prevailed upon to relinquish its unspeakable advantages, unless satisfied by irrefragable arguments of its unsoundness. In the atomic symbols and formulæ of Berzelius, he possesses a universal language, characterized by compactness, simplicity, and precision, without which he could scarcely hope to define complex organic products, or to render distinct their mutual relations and substitutions. In the numbers supposed to represent atomic weights, he has a rigorous standard for controlling individual analyses, and is thus enabled to ascend by the steps of empirical results essentially imperfect, "tantâ stant prædita culpâ," to normal and abstract truth. This principle of control will become more absolute, as the atomic numbers themselves, with the increasing perfection of instruments and greater purity of re-agents, approach more closely ideal exactitude; and would attain an almost mathematical rigour, if future research should realize the master conception of Prout and Thomson, that the atomic weights of all other elements are multiples of that of hydrogen, and may therefore be rigidly expressed by integral numbers. As far, then, as can be safely inferred from the experience of the last fifty years, it seems reasonable to anticipate that the

atomic theory will receive progressive light and growing confirmation from the future advance of science, that in its simplicity and its grandeur it will still prove capable of interpreting and comprehending within its wide embrace all the phenomena of chemical combination hereafter to be revealed; and that it will thus ever maintain "its elevated rank in the scale of physical truths, as after the laws of mechanics, the most important which the study of nature has yet disclosed."

This brief recapitulation of Dalton's main discoveries may enable us to estimate his characteristic mental gifts, and to define his position among the masters of science. Nature had manifestly bestowed upon him, in unstinted measure, the faculty of contemplating the most abstract relations of space and number. He was thus impelled by an irresistible instinct to the study of the pure mathematics, and found or created means for their earnest culture. He came forth thereafter as a labourer, into the field of the experimental sciences, with faculties braced by this healthful discipline, and with a mind stored with mathematical principles, and practised in the severe logic of mathematical reasoning. Profoundly impressed by the success of Newton's efforts to bring under the control of mathematical laws the movements of the heavenly bodies, the principles of mechanics, and even the phenomena of light, Dalton commenced his researches in general physics, with the confident expectation of finding all empirical phenomena governed by similar relations, susceptible of simple mathematical expression. Thus we have seen him striving to prove, that the changes produced in matter, in its two conditions of liquid and elastic fluid, by heat, vary as the square, cube, or other function of the temperature. This mathematical character is deeply impressed on all his workings; on what has perished, as well as on what endures. His inmost mental nature, and all its outward manifestations were, therefore, in the language of the German metaphysicians, emphatically *subjective*. Thus

in special or *objective* chemistry he has left absolutely no sign of his presence; no great monograph on an individual body and its compounds: no memorable analysis of a substance deemed simple, into yet simpler elements; no new element,—no Neptune,—added to the domain of chemistry. His were not the mental endowments, capable of discerning the analogy, in chemical habitudes and *objective* properties between the earths and alkalies, and the metallic oxides, which guided Davy to his greatest discovery. Even after the new metals had been extracted from their alkaline oxides, we have seen him reluctant to recognize their rank as simple elements.

His mental tendencies were thus exclusively to meditation and abstract reasoning. Without venturing to compare him with one who has had no equal or compeer in the highest science, it may yet be affirmed, that his intellectual habitudes were near of kin to those attributed to Newton. Both these great philosophers were characterized by the faculty of steady, prolonged, unswerving attention, wholly abstracted from external objects and events; of patient concentrated thought. It was Dalton's wont, as in boyhood so in mature life, to struggle with the problem he had to solve, silently and intensely in the depths of persistent meditation. He was ever accustomed to maintain that greatness in any pursuit is mainly reached by indomitable perseverance. In his own words, "If I have succeeded better than many who surround me, it has been chiefly, nay, I may say, almost solely from unwearied assiduity. It is not so much from any superior genius that one man possesses over another, but more from attention to study, and perseverance in the objects before them, that some men rise to greater eminence than others." Like all original self-reliant minds, he was never solicitous to consult books, or to learn the opinions of others. In this respect he resembled Dr. Matthew Stewart, who, we are informed by Playfair, "read few books, and verified the observation of M. D'Alembert, that of all men of letters, mathematicians read least of the writings of one another. His own investigations occupied him sufficiently; and indeed

the world would have had reason to regret the misapplication of his talents, had he employed in the mere acquisition of knowledge, that time which he could dedicate to works of invention." My friend, Mr. John Greg, informs me, that when Dalton's pupil, " I asked his advice respecting a book which I wished to buy: he reproved me for my disposition to buy books, and pointing to a few black shelves around his dusty room, said, 'these and as many over the way are all *I* have.' " His extreme reluctance to increase the small scientific library belonging to the Literary and Philosophical Society, and the only one in Manchester, by the purchase of even such important works as the *Mécanique Céleste*, or of any books except the periodical journals, and his preference of all other modes of expending the surplus income; as in copies of the portrait of Newton and other illustrious men, must be fresh in the remembrance of many members of that society.

Dalton's habits of association and reasoning were slow and somewhat laborious, even when he was merely perusing the deductions of other mathematicians. In reading the Principia with him, I observed that he would rarely combine ratios by a mental process, but insisted upon my writing them down on a slate, under one another, and then deliberately effecting their combination. There was nothing fitful or impulsive in his nature; no sudden gleams of inspiration. He was ever calm, thoughtful, passionless. Imagination had absolutely no part in his discoveries: except, perhaps, as enabling him to gaze, in mental vision, upon the ultimate atoms of matter, and as shaping forth those pictorial representations of unseen things, by which his earliest as well as his latest philosophical speculations were illustrated. He had no taste for polite letters, unless his youthful poetical attempts be accepted as evidence of such taste. He was inclined to discourage the reading literary essays before the society over which he presided, the professed objects of which are yet literary as well as philosophical. It was amusing to hear him pass judgment, from the chair, on such essays, in perfect good humour, but in terms somewhat humbling to

their authors, as contributing no positive facts to our stock of knowledge, as in short, "proving nothing." His own style was simple, concise, and masculine, but often quaint and abrupt, and always wholly negligent of ornament.

Such are the impressions which, as his pupil in earlier years, and as honoured by his kindly regard in mature life, I have formed of my venerable instructor and friend; and of the vast impulse given by his genius to the higher philosophy of chemistry. The image must needs be tinged and enfeebled by the imperfect medium through which it has been transmitted. It is no easy task to follow Dalton, with unequal steps, through his wide and lofty range of research and speculation. No one who has not been himself ardently engaged in furthering the onward movement of chemical knowledge, can handle successfully the single theme of the atomic philosophy, and place it in the full light of evidence and illustration which now streams upon it from so many new regions in the ever-widening horizon of science.

Ma io perchè venirvi? o chi 'l concede?

. . . . . .

Me degno a ciò nè io nè altri il crede.

APPENDIX.

Dr. George Wilson, the author of important contributions to the *Edinburgh Monthly Journal of Medical Science*, in the numbers for November, December, and January last, on the "Prevalence of Chromato-Pseudopsis or Colour-Blindness," has obligingly written for me the following interesting statement of his views respecting the nature of Dalton's peculiarity of vision.

The peculiarity of vision in reference to colours which belonged to Dalton is not easily distinguished by a name. Dalton himself seems to have felt this, and left it unnamed; and later writers on the subject have not succeeded in suggesting a generally acceptable term. In truth, as colour-vision may vary in respect to normality in many different ways, a single term fully inclusive of all its peculiarities can scarcely be looked for, until our knowledge of these is much greater than it is at present.

Dalton's paper on the subject was read to the Literary and Philosophical Society of Manchester, on 31st October, 1794, and was published in its *Memoirs* for 1798. It was entitled "Extraordinary Facts relating to the Vision of Colours," and referred to some twenty cases besides the author's own.

The earliest term by which the abnormal colour-vision thus described, appears to have been distinguished, was *Daltonism*, a word introduced by Pierre Prevost, of Geneva, in 1827, and made familiar to English readers by Professor Wartmann.*

Against the introduction of this term, a protest nearly

* "Memoir on Daltonism." By M. Elie Wartmann, Professor of Natural Philosophy in the Academy of Lausanne.—*Taylor's Scientific Memoirs*, 1846, p. 156.

unanimous has been entered in this country, and it is needless to insist on the many objections which apply to it.

I have recorded elsewhere my own strong aversion to it, and here, therefore, I may, without fear of misconception, state, that I cannot help acknowledging that Dalton, however unwittingly, was himself largely the cause of the objectionable term referred to being introduced. His paper contains three divisions: "I. Of My Own Vision. II. An Account of others whose Vision has been found similar to mine. III. Observations tending to point out the Cause of our Anomalous Vision."

The whole cases described are thus grouped round himself by Dalton, as sharers with him of a common peculiarity, and Prevost, in reality, only followed Dalton when he spoke of those who resembled him in their perception of colours as Daltonians. Nor does it appear that this phrase gave offence to Dalton, who was more amused than annoyed with his singularity of vision, and was always ready to satisfy the curiosity of others in reference to it.

Of the names proposed in room of Daltonism, the one which best represents our actual knowledge of its nature is "Chromato-pseudopsis," or the False Vision of Colours; a term sufficiently general to include all the varieties of abnormal Colour-Vision, without committing its employer to any theory as to their cause.

The word, however, is too harsh in sound to come into general use, and even if it were more euphonious, we should still require some expressive English term universally intelligible and acceptable.

The name "Colour-Blindness," proposed by Sir David Brewster, is in this respect unexceptionable, and does not probably overstate the truth; for although the mistaking, *ex. gr.* of pink for blue, or blue for pink, might seem to imply the vision of one colour in room of another, rather than blindness to either or to both, yet the majority of such mistakes admit of a very satisfactory explanation by the assumption that the retina is insensitive to one of the two colours which it misapprehends. There is another name, however, which was employed by Sir John Herschel in corresponding

with Dalton in reference to his vision of colours, which very happily expresses the marked point of difference between a colour-blind and a normal eye. This is *Dichromic*, or, as it may be Englished, *Two-Colour Vision*. A normal eye sees perfectly the three colours, generally accepted as primary, namely, red, yellow, and blue, throughout all their mixtures and shades, so that it exercises Trichromic or Three-Colour Vision. An eye like Dalton's, on the other hand, sees only yellow and blue, and is insensitive to red.

Significant, however, as the term *Dichromic Vision* is, it expresses too much; for amongst a large number of colour-blind persons, several of whom made greater mistakes between colours than Dalton did, I have not found one who did not in favourable circumstances distinguish red alike from yellow and from blue, and the prevailing tendency of their mistakes was not towards a confounding of the full primary colours with each other, but of these with secondary colours, as blue with purple, and red with green.

Further, so far as I have had the opportunity of observing, it has appeared to be an unfailing peculiarity of the colour-blind eye, to be unable to distinguish from each other, the light and the dark shades of *all* colours. There is a certain point for every eye, at which the addition of white or of black to a colour perfectly distinguishable in its full intensity, renders that colour imperceptible; so that the tinge of red, blue, or yellow, *ex. gr.* which is present makes no impression on the retina. But the colour-blind eye has its sensitiveness much more narrowly limited in the direction both of light and of dark shades, than the normal eye; so that pink, pale blue, primrose yellow, and light green, are mistaken for each other and for dirty white, and dark purples, olives, browns, and blues, are confounded with one another, and classed along with black. In reference to the paler and darker shades of colours, a colour-blind eye is thus *Achromic*, whilst towards the full and pure primary colours it is *Dichromic*, or blind to one of the three. Upon the whole, then, the very convenient term Colour-Blindness may be held to express the opinion alike of Sir John Herschel and Sir David Brewster, in reference to the peculiarity of

vision under notice, and I shall accordingly employ it instead of the term chromato-pseudopsis in the following statement.

Dalton was first distinctly convinced of his peculiarity of vision in 1792, by the discovery that the flower of a geranium which appeared to others *pink* in all lights, appeared to him blue by day, and "what he called red," by candlelight. This observation led him to examine into the peculiarities of his vision, and he began with the study of the solar spectrum. In this he saw only *two,* or at most *three* distinctions. "These," he says, "I should call *yellow* and *blue,* or *yellow, blue,* and *purple.* My *yellow* comprehends the *red, orange, yellow,* and *green* of others; and my *blue* and *purple* coincide with theirs."

From this statement it should seem that Dalton saw as long a spectrum as those whose eyes are normal in their perception of colour, although the less refrangible end of the spectrum did not appear to him red. Sir John Herschel, accordingly, in the important letter to Dalton, with a perusal of which I have been favoured, says, "It is clear to me that you and all others so affected, perceive *as light* every ray which others do. The retina *is excited* by every ray which reaches it;" and again, "It seems to me that we have three primary sensations where you have only two. We refer, or can refer in imagination, all colours to three, yellow, red, and blue. To eyes of your kind it seems to me that all your tints are referable to two."

Wartmann, in his paper on Colour-Blindness, after observing that "Dalton sees in the solar spectrum three colours only, yellow, blue, and purple," adds in a note, "Sir David Brewster affirms, on the contrary, in his *Letters on Natural Magic,* that according to his observations, Dalton saw the whole entire spectrum, but that the red portion appeared to him yellow."

The editor of the *Scientific Memoirs,* in which Wartmann's paper appeared (vol. IV. 1846, p. 164), observes, "There is no contrariety whatever between Sir D. Brewster's affirmation and Dr. Dalton's own statement; Dr. Dalton has never stated that the spectrum he saw was *shorter* than the spectrum seen by others."

Sir David Brewster's own words are, "In all those cases which have been carefully studied, at least in three of them, in which I have had the advantage of making personal observations, namely, those of Mr. Troughton, Mr. Dalton, and Mr. Liston, the eye is capable of seeing the whole of the prismatic spectrum, the red space appearing to be yellow. If the red space consisted of homogeneous or simple red rays, we should be led to infer that the eyes in question were not insensible to red light, but were merely incapable of distinguishing between the impressions of red and yellow light. I have lately shown, however, that the prismatic spectrum consists of three equal and coincident spectra of *red, yellow,* and *blue* light, and consequently, that much yellow and a small portion of blue light exist in the red space; and hence it follows, that those eyes which see only two colours, namely, yellow and blue, in the spectrum, are really insensible to the red light of the spectrum, and see only the yellow with the small portion of blue with which the red is mixed. The faintness of the yellow light which is thus seen in the red space, confirms the opinion that the retina has not appreciated the influence of the simple red rays."*

It is with great diffidence that I express dissent from authorities so high as Brewster and Herschel, especially as I must appear also to dissent from Dalton himself, who permitted those philosophers to describe his case, as they have done during his lifetime. It must be remembered, however, that there is no common language between the colour-blind and the colour-seeing, and that Dalton gave in only a silent and negative adhesion to the conclusions which have been quoted.

That Dalton's vision was strictly dichromic appears to me very questionable. I have found no fact in his statement of his own case which decides the point, but I am strongly inclined to think that in favourable circumstances he was not insensible to red as distinct from blue on the one hand, and yellow on the other. The grounds for this conclusion are the following.

* *Letters on Natural Magic,* 4th edition, p. 32.

1. In examining more than twenty colour-blind persons, whose cases presented the same features as that of Dalton, but some of them in a much more marked degree, I found, as already mentioned, none without some power of distinguishing red from yellow and blue coloured objects. Their power in this respect was tested by giving them bundles of coloured wools (and sometimes pieces of coloured glass) to arrange. In assorting them the greatest mistakes were made, as I may best illustrate by giving the details of one case where the party (an intelligent young man) had the wools referred to beside him for a week, and frequently revised his arrangement which was made during daylight.

BERLIN WOOLS AS ARRANGED BY T. R.

Red Bundle.
3 scarlets
1 crimson, rather dark
3 reds, deep
1 orange, chrome
1 brown, ochre
2 greens, rather dark
1 citrine, with excess of green
1 green, dirty

Blue Bundle.
2 blues, bright
2 blues, dark
1 blue, pale
1 purple, dark
2 lilacs, 1 dark and 1 light
1 pink, rose or pale crimson

Brown Bundle.
1 brown, light ochre
2 greens, light

Purple Bundle.
2 crimsons, 1 bluer than the other

Unarranged.
1 black
1 dark green

Green Bundle.
1 green, full and bright
1 green, full but bluish
1 scarlet, full and bright
1 buff, light
2 drab, light
1 drab, dark
1 flesh colour, dirty or dull

Pink Bundle.
1 salmon colour
1 peach blossom
2 blues, light

Orange Bundle.
1 orange, full and bright
1 yellow, dull
1 green, pale
1 citrine

Fawn Bundle.
1 pale green

Yellow Bundle.
1 pale yellow

In this arrangement it will be seen, that neither blues nor yellows appear in the so-called red bundle, and that seven of the thirteen wools in it are red. Neither do any reds appear in the so-called blue or yellow bundles, with the

exception of one pink in the blue, whilst two crimsons are marked as purple, and one scarlet, and one dirty flesh colour, appear among the greens. I have particularized this case simply because his arrangement of colours was more carefully noted than that made by any other colour-blind person known to me. Long, however, before I was introduced to the party in question, and before I had formed any theory on the matter, I had had the unexpected fact forced upon my observation, that those who made the greatest mistakes between red and green, and whose account of themselves was, that they could not distinguish red from other colours, nevertheless did place the greater number of the reds in the right parcel, and although greens and browns found their way into the so-called red bundle, reds formed the majority in every case; whilst in like manner, in the green bundle, the majority of wools were green, and only a very few red.

Further in watching a colour-blind person arranging wools, I have generally observed him in selecting reds, go on adding red skein to red skein, slowly and somewhat hesitatingly but without any mistake, till some six or seven had been placed together. Then apparently the eye lost its sensitiveness to red, and a wool of a different colour, generally a green, was taken, and after that no return was made to red.

From these observations I cannot help inferring that red makes an impression of the same kind on a colour-blind as on a normal eye, but that it is much less decided and abiding on the former than on the latter, so that the vision of both, though in different degrees is Trichromic. There is nothing in the record of Dalton's case to show that he was an exception to his colour-blind brethren in this respect, but as it does not appear that he was tested as to his power of distinguishing colours in the way I have described, it is impossible to say more than that it cannot be held as certain that he had not a positive perception of red.

It may be argued that the coloured wools did not present single or pure colours, and that the impression which they made upon the retina resulted from the perception of yellow, blue, or white light to which the eyes of the colour-blind

are confessedly sensitive. That the colours of the wools were very mixed is not to be questioned, but the acknowledgment of this does not invalidate the important observation, that where the worsted had one colour so predominant that by the possessor of a normal eye it was pronounced without hesitation to be red, the same name was given to it in many cases by one who was colour-blind.

Further when red was mistaken for other colours, it was not for yellow in any case that has come under my notice, but generally for green or brown.

The fact that green is so often supposed to be the colour offered to the eye, when it is red that is before it, appears at first sight to confirm Sir D. Brewster's belief, that in the red end of the spectrum, the colour-blind see only the yellow and blue, which, according to his view of a three-coloured spectrum, they only can see, if the red is invisible to them. But the green producible by the pale yellow and very pale blue of the less refrangible end of such a spectrum, would be a very faint yellow-green: whereas reds, scarlets, and crimsons are confounded by the colour-blind with the deepest full greens. It will suffice to refer to Dalton alone, who has recorded that the one side of a laurel-leaf seemed to him "a good match to a stick of sealing-wax" and the other side to a red wafer.

Again, if the red end of the spectrum appeared yellow to Dalton, one should expect yellow objects to be confounded with those which were red, but under the heading of "Orange and Yellow by Daylight and Candlelight," he says, "I do not find that I differ materially from other persons in regard to these colours. I have sometimes seen persons hesitate whether a thing was white or yellow by candlelight, when to me there was no doubt at all."—(*Manch. Mem.* 1798, p. 34.)

In conformity with this observation, I have found the colour-blind whom I have examined almost invariably right in their identification of yellow. Yellow-orange they call yellow and sometimes green, and it is only deep red-orange, approaching to scarlet, which they identify with red, and confound with the colours (especially green) with which they confound red.

Nothing that has been urged could countervail Dalton's deliberate declaration that he had ascertained that he did not see red as red, and that he saw it as yellow. He does not, however, anywhere declare or imply, so far as I can discover, that such was the case. Under the head of "Green" (*Manch. Mem.* 1798, p. 34), he says, "It will be immediately concluded, that I see either red or green, or both, different from other people. The fact is, I believe that they both appear different to me from what they do to others. Green and orange have much affinity also." From this passage it should seem that Dalton held his perception of red to be peculiar, and the only statement throwing further light on his positive appreciation of that colour, is the following striking and important one, in reference to the solar spectrum. After stating that he sees in it at most three distinctions of colour, *yellow*, *blue*, and *purple*, he continues, "That part of the image which others call red, appears to me little more than a shade or defect of light; after that the orange, yellow, and green seem *one* colour, which descends pretty uniformly from an intense to a rare yellow, making what I should call different shades of yellow."

Here Dalton, whilst he emphatically identifies orange and green with yellow (the term "one" being italicised by him), excludes red from this identification, and speaks of it as appearing defectively illuminated or dark, *i.e.*, more or less *black*.

And in conformity with this perception of the spectral red as "a shade or defect of light," Dalton shows by his references, that although from trials with vermilion he was quite certain that he never saw it black, which from his theory of colour-blindness he thought he should have done, yet certain reds appeared to him dark and at least verged upon blackness. Thus he says, *Op. Ct.* pp. 32, 33, (the italics are his own), "All crimsons appear to me to consist chiefly of dark blue; but many of them seem to have a strong tinge of dark brown. I have seen specimens of *crimson*, *claret*, and *mud* which were very nearly alike." Again, "Crimson has a *grave* appearance, being the reverse of every showy and splendid colour. Woollen yarn dyed crimson or

dark blue is the same to me." Again, "The colour of a florid complexion appears to me that of a dull, opaque, blackish-blue upon a white ground. A solution of sulphate of iron in the tincture of galls (that is dilute black ink), upon white paper, gives a colour much resembling that of a florid complexion. It has no resemblance to the colour of blood." . . . . . "Stockings spotted with blood or with dirt would scarcely be distinguishable." Lastly, "By day some reds are the least showy imaginable: I should call them dark drabs."

Dalton thus appears, in conformity with his perception of the red end of the spectrum as dark or darkish, to have seen certain red objects as dark blue, dark brown, dark drab, mud-coloured, dirt-coloured, and even like ink. I feel, therefore, compelled to conclude, that he had not a *constant* perception of so long a spectrum as those with normal eyes have, and that red occasionally, if not frequently, failed to make either a luminous or a colorific impression on his eye, or at least made so faint an impression, that he likened the sensation which it occasioned to the absence of light and the absence of colour.

It is a satisfaction to me in stating this result to feel that it is in several important points in unison with the conclusions of Herschel and Brewster. It is at one with the former's belief that Dalton and those like him perceive in favourable circumstances "as light every ray which others do," whilst I would add to this the statement that, in the same circumstances, they are as conscious as others of three primary sensations (red, yellow, and blue) of colour. It is at one also with Brewster's inference that "the retina does not appreciate the influence of the simple red rays."

In short, I agree with those great observers in the conclusion that the vision of Dalton, and of the colour-blind in general, is prevalently dichromic, whilst I differ from them in thinking that it is occasionally trichromic.

It does not seem necessary to discuss here Dalton's own theory of colour-blindness, as his supposition that the vitreous humour of his eye was blue is known now to have been unfounded.

I will only add that the perception of pink and of crimson as blue, appears to be an almost uniform peculiarity of the colour-blind. It perplexed me long to explain why they should speak even of a light pink as a decided blue, but I believe that the explanation of this will be found in the fact that the red in a mixture of red and blue does not affect the retina, *but appears black*, so that pink does not appear to them as if it were simply deprived of the red in it, but as it would show to those with normal eyes if the red were replaced by black.

* A more detailed statement of the views contained in the text, and of others referring to kindred topics, will be found in part in the recent numbers of the *Edinburgh Monthly Journal of Medical Science*, and will appear in full in a republication from that periodical.—G. W.

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REPORT

OF

THE SEVENTH ANNIVERSARY MEETING

OF THE

CAVENDISH SOCIETY.

The Anniversary Meeting of the Cavendish Society for the year 1854 was held at the rooms of the Chemical Society, No. 5, Cavendish Square, on Wednesday, the 1st of March, at three o'clock in the afternoon.

The Chair was taken by Thomas Graham, Esq., F.R.S., President, who called upon the Secretary to read

THE REPORT OF THE COUNCIL.

"It becomes the duty of the Council, on the recurrence of the Anniversary Meeting, to report the results of their proceedings for the past year to the Members of the Cavendish Society. The objects of the Society continue to be steadily carried out, and the advantages resulting from its operations in causing a considerable accession to our scientific literature, receive increased recognition as the number of the Society's works is augmented, and their merits become more generally known and appreciated.

"The eighth volume of Gmelin's 'Hand-book of Chemistry' has been supplied to the Members as one of the books for 1853. This being the second volume of the part of the work which treats of Organic Chemistry, the Council have been glad to find that it continues to be received by the Members with undiminished satisfaction. The great merit of the work consists in the concise, methodical, and explicit manner in which the facts and theories of the science are described. The Council consider that they have been fortunate in having committed the duty of editing the English edition to one so competent, as Mr. Watts has proved himself to be, to co-operate with the author in rendering the work complete by the introduction of new matter. The death of Professor Gmelin, which has occurred during the past year, is calculated to increase the responsibilities of the Editor; for although the author had completed a considerable portion of the new edition of the work beyond what has been translated, yet much remains in an unfinished state, the arrangements for the completion of which have not been announced. There can be no doubt, however, that means will be adopted for making the concluding part of this great work worthy of the distinguished author whose name it bears.

"The first volume of BISCHOF'S 'Elements of Chemical and Physical Geology' is the second book prepared for 1853. The delay which has occurred in the issuing of this volume has been caused by circumstances which the Council could not control.

"The third and last volume of the translation of LEHMANN'S 'Physiological Chemistry,' by DR. DAY, is in a forward state, and this will be one of the books for 1854.

"Among the objects originally contemplated by the Society, was the publication of the biographies of distinguished Chemists. The Council have, for some time past, been anxious to add to the life of CAVENDISH, among the works of the Society, the lives of DALTON and WOLLASTON, and they are now enabled to state that DR. HENRY has written a biography of DALTON, which he has liberally placed at the disposal of the Council. This work is nearly printed, and will very shortly be supplied to the Members as a book for 1854, for which year, in addition to the works already mentioned, a third volume will be prepared.

"In the publication of GMELIN'S 'Hand-book of Chemistry' by the Society, it was originally contemplated that the demand for this work would continue for many years, and, in anticipation of this, a larger number of copies were printed than have hitherto been required for supplying the Members of the Society. Independently of the copies which will be kept for completing the sets belonging to Members who have not yet obtained all the books of the Society, there still remain on hand about three hundred complete sets of the first six volumes, comprising the whole of that part of the work which treats of Inorganic Chemistry. The Council have had under consideration the best mode of disposing of this part of their stock, and being of opinion that there are many Chemists throughout the country who, although not subscribers to the Society, would be anxious to possess this valuable work, they have thought it desirable that an opportunity should be offered to such, and also to public libraries, to obtain the volumes referred to through Members of the Society. It is therefore proposed that the first six volumes of GMELIN'S 'Hand-book of Chemistry' be supplied to Members of the Cavendish Society who have already subscribed for the years 1849, 1850, and 1851, at two guineas a set; and if this proposition should obtain the sanction of the Meeting, a resolution to that effect will be immediately put into operation. The Council conceive that by this means the interests of the Members will be promoted, and a further extension than has hitherto been effected will be given to one of the principal objects of the Cavendish Society, which is to bring valuable chemical works, not otherwise attainable, within the reach of English Chemists."

Treasurer's Statement of the Receipts and Expenditure of the Cavendish Society, from the 1st of March, 1853, to the 27th of February, 1854.

Receipts.

			£	s.	d.
Balance from previous year			594	4	1
31	Subscriptions for	1848	17	6	6
31	Ditto	1849	32	11	0
41	Ditto	1850	43	1	0
63	Ditto	1851	66	3	0
161	Ditto	1852	169	1	0
345	Ditto	1853	362	5	0
80	Ditto	1854	84	0	0
			£1368	11	7

Expenditure.

	£	s.	d.
Stationery, Postage, Delivery of Books, &c.	23	11	8
Advertisements	6	5	0
Insurance	2	5	0
Collector's Commission	10	17	0
Secretary	100	0	0
Editorial expenses	239	19	3
Paper	153	9	0
Printing and Engravings	394	1	6
Binding and wrapping	105	6	1
	1035	14	6
Balance in hand	332	17	1
	£1368	11	7

We have examined the above statement, and find it correct.

February 27th, 1854.

DUGALD CAMPBELL.
G. B. BUCKTON.

It was resolved,

"That the Report just read be received, approved, and adopted."

The Meeting then proceeded to the election of Officers for the ensuing year, and the following Gentlemen were declared to have been duly elected:—

President.

Professor Graham, F.R.S.

Vice-Presidents.

Jacob Bell, F.L.S.
Professor Brande, F.R.S.
Earl of Burlington, F.R.S.
Sir James Clark, M.D., F.R.S.
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W. A. Miller, M.D., F.R.S.
Professor Wheatstone, F.R.S.
Colonel Philip Yorke, F.R.S.

Council.

G. B. Buckton, F.C.S.
Dugald Campbell, F.C.S.
Philip James Chabot, M.A., F.C.S.
W. Ferguson, F.C.S.
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James Tennant, F.C.S.
Charles Tomlinson, Esq.
A. W. Williamson, Ph.D., F.C.S.

Treasurer.

Henry Beaumont Leeson, M.D., F.R.S., St. Thomas's Hospital.

Secretary.

Theophilus Redwood, Esq., 19, Montague Street, Russell Square.

It was resolved,

"That Dr. J. H. Gladstone, Mr. Henry Deane, and Dr. Garrod, be appointed Auditors for the ensuing year."

The following Resolutions were unanimously adopted:—

"That the thanks of the Meeting be given to the President, Treasurer, and Council, for their services to the Society."

"That the thanks of the Meeting be given to the Honorary Local Secretaries for their services to the Society."

"That the thanks of the Meeting be given to the Chemical Society for the use of their rooms on the present occasion."

The Meeting was then adjourned.

THEOPHILUS REDWOOD, Secretary,
17, Bloomsbury Square.

March 1st, 1854.

WORKS OF THE CAVENDISH SOCIETY.

1848.

1.—CHEMICAL REPORTS AND MEMOIRS. Edited by Thomas Graham, F.R.S. (Out of Print.)

2.—HAND-BOOK OF CHEMISTRY. By Leopold Gmelin. Translated by Henry Watts, B.A., F.C.S. Vol. I.

1849.

3.—HAND-BOOK OF CHEMISTRY. By Leopold Gmelin. Vol. II.

4.—HAND-BOOK OF CHEMISTRY. By Leopold Gmelin. Vol. III.

5.—THE LIFE AND WORKS OF CAVENDISH. By Dr. George Wilson.

1850.

6.—HAND-BOOK OF CHEMISTRY. By Leopold Gmelin. Vol. IV.

7.—HAND-BOOK OF CHEMISTRY. By Leopold Gmelin. Vol. V.

1851.

8.—PHYSIOLOGICAL CHEMISTRY. By Professor Lehmann. Translated by George E. Day, M.D., F.R.S. Vol. I.

9.—HAND-BOOK OF CHEMISTRY. By Leopold Gmelin. Vol. VI.

1852.

10.—HAND-BOOK OF CHEMISTRY. By Leopold Gmelin. Vol. VII. (Organic Chemistry, Vol. I.)

11.—PHYSIOLOGICAL CHEMISTRY. By Professor Lehmann. Vol. II.

12.—ATLAS OF PLATES relating to PHYSIOLOGICAL CHEMISTRY. By Dr. Otto Funke.
(Supplement to Lehmann's Physiological Chemistry.)

1853.

13.—HAND-BOOK OF CHEMISTRY. By Leopold Gmelin. Vol. VIII. (Organic Chemistry, Vol. II.)

14.—ELEMENTS OF CHEMICAL AND PHYSICAL GEOLOGY. By Professor Bischof. Vol. I.

The first of the Society's publications, the volume of Chemical Reports and Memoirs, being out of print, those who now join the Society may be supplied with the first volume of Gmelin's Chemistry, on payment of half the Subscription for 1848. Members commencing from 1851 have the option of taking The Life of Cavendish instead of the sixth volume of Gmelin's Chemistry as the book which is given in addition to the first volume of Lehmann's Physiological Chemistry for that year. The eighth volume of Gmelin's Chemistry may be substituted for Lehmann's Physiological Chemistry, and the Atlas of Plates, for the year 1852.

Members of the Cavendish Society who have already subscribed for the years 1849, 1850, and 1851, may obtain copies of the first six volumes of Gmelin's Hand-Book of Chemistry, comprising the whole of the Inorganic Chemistry, for two guineas.

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